SCIENTIFIC METHOD IN PTOLEMY'S 'HARMONICS'

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The science called 'harmonics' was one of the major intellectual enterprises of Greek antiquity. Ptolemy's treatise seeks to invest it with new scientific rigour; its consistently sophisticated procedural self-awareness marks it as a key text in the history of science. This book is the first sustained methodological exploration of Ptolemy's project. After an analysis of his explicit pronouncements on the science's aims and the methods appropriate to it, it examines Ptolemy's conduct of his complex investigation in detail, concluding that despite occasional uncertainties, the declared procedure is followed with remarkable fidelity. Ptolemy pursues tenaciously his novel objective of integrating closely the project's theoretical and empirical phases and shows astonishing mastery of the concept, the design and (it is argued) the conduct of controlled experimental tests. By opening up this neglected text to historians of science, the book aims to provide a fresh point of departure for wider studies of Greek scientific method.

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Preface

During the 1970s and 80s, it was my regular habit to take Philosophy undergraduates at the University of Warwick on a guided tour around a selection of Platonic and Aristotelian texts; and I generally found myself placing issues about the nature of knowledge, and about the procedures by which it may be pursued, firmly at the centre of our agenda. I became more and more fascinated, in the course of this annual pilgrimage through *Meno, Phaedo, Republic, Theaetetus, Posterior Analytics, Physics* and *Nicomachean Ethics*, by their intricate negotiations between what we would call 'rationalist' and 'empiricist' conceptions of the route towards knowledge in a variety of different fields of enquiry. In 1976 the University of Warwick allowed me to accept an invitation to spend two years teaching in the Faculty of Classics at Cambridge; and it was there, with my mind full of these matters, that I first stumbled, largely by accident, into the thickets of the Greek musical sciences. As I worked backwards from Aristoxenus to Plato and the early Pythagoreans, and then forwards into later antiquity, I discovered that the surviving texts of that unfamiliar tradition can be read as a record of continual controversy, not so much over musicological details as over the general character of the understanding sought by scientists in this field, the methods by which it is to be pursued and secured, and the relations that hold between the propositions of this science and those belonging to other domains. The attempt to unravel the complexities of these debates has occupied me, with a few intermissions, ever since. I translated Ptolemy's *Harmonics* during the late 1980s as part of the material for the second volume of my *Greek Musical Writings*, and the more I studied it the clearer it became that it is a landmark of major significance in the contentious and quarrelsome history of reflections on scientific method. I realised that it called for much fuller examination, from a methodological perspective, than I could possibly give it in the context of that book.

I set out on the project in 1991. A period of leave from the University of Warwick gave me the opportunity to take up a Visiting Fellowship in the Department of Classics at the University of Queensland, where I had the

leisure to work out the plan for this study and to write some extensive drafts. Over the next eight years, as I abandoned the philosophers at Warwick to join the classicists at the University of Otago, and later left Otago for Birmingham, among the distractions of administrative duties and the seductions of other research enterprises that came my way, the book progressed only by fits and starts; and it was not until the turn of the millennium that it was ready to be dropped into the lap of its amazingly tolerant publishers.

I am very grateful to the Cambridge University Press and its staff, especially Pauline Hire, for the patience they have shown, as well as for their familiar diligence and efficiency. Many thanks also to my admirable copy-editor, Muriel Hall, who read the long typescript with meticulous attention, and alerted me to a number of potentially embarrassing mistakes and obscurities. I have done my best to eliminate the former and resolve the latter; responsibility for those that remain should of course be laid at my own door. I am grateful, too, to my colleagues and students in all the universities I have mentioned for their friendship, for the conversations I have had with them over the years, and for their willingness to take an interest in my sometimes esoteric obsessions. Issues investigated in this book have been the subject of papers I have delivered at conferences and seminars in England, Australia, New Zealand, Canada, France and Italy, and I owe a great deal to the scholars who took part in discussions on those occasions. Special thanks are due to Geoffrey Lloyd, Malcolm Schofield, Tony Long, David Fowler and Annie Bélis for their long-standing encouragement of my work in this field. The intellectual stimulation and personal support I have drawn from the experience of sharing my life with my wife, Jill, has been worth more to me in this and all my other activities than I can possibly say; and she, together with our children, has done much to keep my feet somewhere near the ground. But I should like to end by expressing my particular thanks to Don and Merle Newman for the many and various pleasures of their inimitable company, and to Ross Newman, Krishna, Vidya and David, in the recesses of whose Brisbane basement so much of the spadework on this project was done, for the generosity with which they gave me the freedom of their remarkable household and Ross's almost equally remarkable car, and for their continuing friendship.

1 Introduction

Ptolemy's reputation as one of the outstanding scientists of the ancient world rests mainly on his epoch-making treatise in astronomy, the *Mathematike Syntaxis* (usually known as the *Almagest*). Modern students of the ancient sciences know him also for his writings on optics, geography and astrology; but with a few honourable exceptions they have shown rather little interest in his *Harmonics*. Those who have examined it in detail have not, for the most part, been historians of science. Either they have been concerned less with the text in its original setting than with its afterlife in Renaissance musicology, or else they are dedicated (even fanatical) specialists in ancient musical theory; and we are rather few. But even if the subject it addresses continues to languish (as it should not) in a cobwebby corner of our gallery of the Greek sciences, the *Harmonics* itself deserves much wider attention.[1]

It is in the first place a work of real intellectual distinction, and its skilfully mustered arguments, despite their technical intricacies, are presented with a flair and panache that should commend it to any connoisseur of scientific writing. Secondly, it is quite unusually explicit and self-conscious about its own methodology and procedures. In this respect it has a good deal more to offer than the *Syntaxis*, whose overt reflections on the general features of the science are relatively brief and less directly methodological, and play a notably less prominent role in the development of the subsequent argument. The *Harmonics*, by contrast, announces and seeks to justify at the outset a sophisticated set of procedural principles which scientists in this field, so it argues, must follow if they are to produce defensible results. It also repeatedly reminds us of these principles in the course of the investigation itself, alerting readers to the ways in which each of its stages fits into the prescribed pattern of procedures, or why it is needed to ensure that the procedures are carried

[1] The problems posed by the text will be significantly eased by a detailed, scholarly study that has just been published (Solomon (2000)), but which reached me, unfortunately, too late to be taken into account while I was writing this book.

through effectively and reliably. Neither the *Syntaxis* nor even the essay *On the Criterion*, devoted though it is to issues concerning scientific understanding and the means by which it should be sought and assessed, gives such clear insights into Ptolemy's conception of the methods appropriate to a science, and of the presuppositions on which its enquiries rest.

The task I have set myself in this book is to explore the *Harmonics* from a methodological point of view. Its own pronouncements on these matters are of great interest in their own right, and demand close analysis. But it will also be necessary to ask how far the treatise is faithful to the principles it advertises, in the actual conduct of its investigations. There are grounds for some scepticism here, and special reasons why the issue should be thought important. The complex combination of rationalism and empiricism which Ptolemy professes to adopt insists, among other things, on a crucial role for experimental tests of provisional, theory-based results. Here, as we shall see, the word 'experimental' is to be construed in a strict sense that will seem surprisingly modern. I hope to show beyond reasonable doubt that Ptolemy understood very well what conditions must be met if experimental tests are to be fully rigorous, and that he had a clear and persuasive conception of the roles they should be assigned in a well conducted scientific project. I do not think that these ideas are so fully worked out and so lucidly expressed in any other surviving Greek source. What is much harder to decide is whether the experimental equipment he meticulously describes was ever actually built, whether his carefully designed and controlled experiments were ever conducted, and if they were, whether he allowed their results genuinely to modify or to put at risk the theoretically grounded conclusions which they purported to test. Greek science in general is not renowned for its adherence to experimental methods. Harmonic scientists in particular often claim that their theoretical results are confirmed 'by perception', sometimes offering geometrically conceived descriptions of instrumental devices through which (they allege) these results can be presented to the ear. But their remarks seldom inspire much confidence in the supposition that the instruments were actually built and used, still less that they were used in an experimental spirit; they seem to have been thought of, at the most, as making manifest 'rationally' excogitated truths to the senses, rather than as putting them to the test. If we are to conclude that Ptolemy not only represented the use of strict experimental techniques as an essential element in a well conducted scientific project, but also carried his programme through in practice, the case will have to be argued in detail and with the greatest caution. Certainly the author's explicit statements about his own procedures should not be taken at face value without a good deal of supporting evidence.

To anticipate the book's conclusions on this issue, I believe that a very strong case can be made in Ptolemy's favour, and I shall do my best to provide it. If it can indeed be shown that when he wrote the *Harmonics*, Ptolemy not only had a well honed understanding of experimental methods but was also seriously committed to their use, that fact should obviously provoke the question whether this treatise is merely a freakish anticipation of later concepts of science, or whether once these methods have been drawn to our notice, we shall be able to find convincing traces of comparable procedures in other Greek works in the 'exact' or 'mathematical' sciences. Such questions have of course been asked before; but it may be that a starting-point in the *Harmonics*, where the issues are brought so insistently to our attention, will place them in a fresh perspective. My business in this book is only to provide the necessary point of departure. No doubt the wider questions are the more important, but they must be reserved for a different book and probably for a different writer. Here I intend to keep the focus as sharp as possible, restricting myself to an examination of this single text, without drawing elaborate comparisons or attempting to generate large conclusions about Greek science in general.

Ptolemy's treatment of the strictly 'rational' or 'theoretical' phases of his enquiry also raises issues relevant to the other sciences, particularly, perhaps, to astronomy. He proposes that what the ear perceives as musically admirable relations between pitched sounds are manifestations of mathematically intelligible and elegant form; and their complex and various structures can be derived, through orderly mathematical procedures, from principles of 'reason' whose credentials are accessible to the mind. This is all very fine and inspiring. But given that our initial data are simply patterns of sound which are perceived as musically satisfactory, we must plainly ask, first, how we are to represent them in ways that express their mathematical form and make them amenable to 'rational' (that is, mathematical) manipulation. We must also ask how we are to move from our initial perceptions to a grasp on the principles which govern their orderly relationships; why it is that these rational principles and no others are the appropriate ones for the task; why it is that some mathematically describable patterns are 'better' and correspond to 'finer' musical relations than others; by what procedures well formed musical systems are to be derived from the initial principles, and why (since different methods of derivation will yield different results); and so on. All these questions have their counterparts in astronomy, at least as Ptolemy conceived it, and the answers offered in the *Harmonics* may shed some light on the character of his reasoning in the *Syntaxis*, perhaps on that of other ancient astronomers too. But those issues, once again, will not be addressed in this book.

From time to time in the course of this study it will be necessary to examine rather closely some of the finer details of Ptolemy's arguments, partly for the light they shed on the nature and application of his method, and partly for their own intrinsic interest. The procedure of the *Harmonics* depends to a high degree on rigorous reasoning, and its sophisticated intricacies can on occasion provide matter for serious philosophical reflection. I shall also suggest, on the other hand, that some of his arguments fail to pass muster by the standards he purports to accept. Some of his constructive strategies seem to break down in their applications; and his criticisms of his predecessors are sometimes more rhetorically than rationally persuasive. It is not always possible to judge whether Ptolemy is merely being too hasty, or whether on some occasions he is deliberately seeking to mislead. This book is a discussion of procedures and the principles governing them, not primarily of the substance of Ptolemy's conclusions or those of other writers in this field; but it would hardly be possible to explore the issues at which I have gestured, at least in any depth, without introducing some musicological technicalities. I shall expound them, however, only to the extent that seems necessary for my main purposes, and a good deal of detail will be ignored. I shall also do my best to introduce them in ways that will be accessible to readers unversed in the conundrums of Greek harmonics, and digestible by those for whom the subject is not itself of special interest.

Because of the narrow restrictions I have placed on my project, and because the *Harmonics* has previously received little close attention from a methodological point of view, this study will seldom engage directly with the work of other modern students of Greek science. I have of course learned a great deal from them. But readers of this book will find it alarmingly free of the reassuring paraphernalia of scholarly footnotes, acknowledgements and backbiting which have become normal in academic literature. I expect to be taken to task for these omissions, but I do not apologise for them. My aim is simply to focus all attention squarely on the contents of Ptolemy's text, with as few distractions as possible.

Greek harmonic science studies a variety of topics, none of which has more than a tangential connection with 'harmony' in the modern English sense of that word. Its general field of operations is melody, the musical sequence of a single melodic line rather than an array, or a sequence of arrays, of simultaneous sounds; and the line's musical credentials and character are conceived – and were heard – as depending solely on its own structure, not on its relation to any accompanying sonorous events, real or implied. Chordal harmony, harmonic progressions and so on are notions quite foreign to the Greek experience. At the centre of the concerns of the

science is the analysis of the elements from which musical attunements are constructed, and of the systematic patterns of relationships in which these elements are bound up to form an organised structure. As a rough preliminary guide, we may conceive an 'attunement' as the system of relations holding between the pitches of the strings of a lyre, for example, when they have been so adjusted that a musical melody can be played on them accurately, with all its intervals 'in tune'. It provides the pattern of pitches and intervals upon which the melody will draw.

A well formed attunement differs from a random collection of pitches in two principal ways. There are limitations, most obviously, on the ways in which adjacent pitches can be related to one another, that is, on the sizes of the intervals that can lie between them, and which a musician can use as basic melodic 'steps'. Though the Greeks admitted in their music a much greater variety of elementary, 'scalar' intervals than are found in later Western practice, not every physically constructible interval was recognised as capable of taking an aesthetically intelligible role in a melodic sequence. There is a distinction to be drawn, then, between musical and unmusical intervals. Secondly, not every possible arrangement of melodically admissible intervals constitutes a well formed attunement. A series of pitches becomes a melody only when they are located within a structure of relations which is recognised as musically coherent; and this structure must be exhibited in the attunement on which the melody is based. Different patterns of relations between pitches and intervals constitute different forms of attunement, each of which is the matrix for a different class of melodies. Several types, distinguished according to criteria of more than one sort, were regularly employed by Greek musicians. But their diversity, like that of the elementary intervals, was not unlimited. Only some arrangements of intervals could be accepted as exhibiting the structure of a musical attunement.

From this perspective, harmonic science had two fundamental tasks. The first is to identify with the greatest possible precision those intervals and structures from which attunements are constituted, marking out the boundaries between them and the realm of the unmusical. The second is to look for the principles on which this distinction between the musical and the unmusical is founded, principles to which a musically acceptable arrangement of intervals must conform. In calling these enquiries 'first' and 'second' I do not mean to imply anything about the order in which they are to be undertaken, or about the logical or epistemological priority of the one over the other. These were issues on which opinions differed; and for Ptolemy, as we shall see, the postulation of principles is in the most important respects prior to the accurate identification of musical intervals.

The part of the programme which I have called 'the first' could be extended in a number of directions, most importantly into enquiries about the ways in which systems of intervals could be grouped and understood in relation to one another – the ways in which small intervallic structures, for instance, could be linked in a series, and the ways in which they could not; or again, the criteria according to which certain structures should be conceived as fundamental and others as derived from them by acceptable processes of transformation. As to the enquiry into principles, Greek theorists of all persuasions were confident that such principles exist, that the differences between the musical and the unmusical are objective and accessible to scientific enquiry, and that they will be found to be orderly and intelligible. They are not dependent on temporary social conventions, or on whim or personal taste, though we, as imperfect human individuals and ones whose perceptions are partly conditioned by our education and social experience, may disagree about where the boundaries lie. If we do, that shows only that we are unreliable judges in the matter, not that the boundaries themselves are flexible or vague. But there was much dispute about the identity of these governing principles, and more radical dispute about the general character of principles proper to the field of harmonics, and of the domain to which they belong.

Though many nuances of opinion on this issue existed, they fell broadly into two groups. Some theorists, notably Aristoxenus and his followers, held that musical principles are autonomous, specific to music itself, not applications to a particular subject matter of laws holding in a wider domain. Others, in a tradition associated with Plato and with a school of thought loosely labelled 'Pythagorean', took the opposite view. Conceiving the relations between pitches in an attunement as essentially quantitative, and the structures formed by them as constituted by relations between quantities, they looked to the science of quantity, mathematics, for the principles by which 'harmoniously' coordinated systems of relations are distinguished from incoherent jumbles. These principles were located specifically in number-theory and in the theory of ratio and proportion. On this latter approach, musical principles are not autonomous. They are to be understood as applications or special cases of principles governing quantities in general, and referring in the first instance to numbers. The perceptible harmoniousness of a musical system is thus a reflection of the intelligible, mathematical coherence of the pattern of quantitative relations between its elements.

This distinction must be explored a little further and linked with certain others. For Aristoxenus and his successors, and perhaps also for those earlier theorists whom he called *harmonikoi*, melodic relations are

characterised by their conformity to principles specific to the musical domain. This means that the forms of coherence exhibited by acceptable attunements, the rules governing musical successions of intervals, and so on, are not ones that hold in a wider field and apply to musical relations because they fall within it, or because they constitute one of various modes of being in which its forms are instantiated. Specifically, relations between pitches are not to be conceived, for the purposes of musical analysis, as relations between quantities, subject to principles of organisation drawn from mathematics. The reason is essentially a simple one. Melody exists in a dimension accessible only to the hearing, and it exists in forms of relation that the ear is capable of recognising. Even if it is true from the perspective of physics that sounds are disturbances in the air, and that they differ in pitch through quantifiable variations in some aspect of these disturbances, it cannot be the relations between these quantities, as such, that give certain sound-patterns their character as melodic, since in hearing them as constituting melody we do not perceive their pitches in the guise of things differing in quantity. If the relations between the notes of music were essentially relations between quantities, then since we do not hear their differences in pitch *as* differences in quantity, we would be unable to hear anything at all as constituting a melody.

Aristoxenus studied with Aristotle, and seems to have been much influenced by his work in metaphysics and the philosophy of science. Here a rudimentary sub-Aristotelian analogy may help. Melody is made up of pitched sounds; they are the 'matter' out of which it is put together. Animals, similarly, are made of certain material stuffs, ones which are compounds, at the most basic level, of the four material elements. Some of the properties of animals are due to their material constitution. But to be a tiger or a kangaroo is not merely to be an assemblage of these elements in some special combination. A kangaroo has properties that are not derived from those of its material constituents, but depend on its possession of a 'nature' additional to theirs, one that actively determines the organism's structure, the course of its development to a determinate mode of completion, and the forms of activity characteristic of its kind. Just so, according to Aristoxenus, though not, as it happens, according to Aristotle himself, 'the melodic' or 'the well attuned' is a *phusis*, a nature, a form of reality independent of all others and obedient to principles peculiar to itself; it organises its materials, sounds, according to rules and patterns of its own. Studies in the physics of sound, that is, of the properties of the materials out of which melodies are made, will no more inform us of what it is to be a melody than studies of the properties of the four elements can reveal what it is to be a kangaroo, and what principles govern such a creature's organisation and behaviour.

Important consequences flow from this position. One is that in a broad sense the science of harmonics must be empirical in its procedures, drawing on the phenomena accessible to the sense of hearing for its data, seeking the principles that govern them through some form of induction or abstraction from these data, and assessing general hypotheses by means of empirical tests. It cannot proceed by seeking to derive them, by reasoning, from principles of an allegedly higher sort. There are no higher principles from which it follows that melody must have the form and nature which it does, any more than there are principles allowing us to derive the special properties of kangaroos from theorems holding of some more inclusive class of beings. Aristoxenus is not an 'empiricist' in the crude sense in which some of his predecessors apparently were. Nevertheless, he insists on the authority of perception as the criterion of what is and what is not harmonically correct. In the end, we can have no reason to believe that this or that principle must necessarily hold, except that attentive listening and reflection on what we hear convince us that it actually does.

A second consequence has to do with the language in which harmonic science is to be articulated and the concepts within which it is framed. If melody is an autonomous phenomenon existing only in the realm of what is heard, the way in which the phenomena are described must reflect the manner in which they strike the ear when they are perceived *as* melody. Musical relations hold between sounds in their guise as audible items, and melodic relations hold only in respect of those aspects of their audible character which affect our perception of them as melodic or unmelodic. For the latter reason, some features of sounds, such as their loudness or brightness, are irrelevant to harmonics even though they are audible; the same melodic relations are involved irrespective of volume or timbre. For the former reason, and more importantly, there is nothing to be learned in harmonics from aspects of sounds that are not perceived as such by the ear, in particular from their character as movements of air which differ in some quantitative manner. Hence sounds conceived under that description have no place in harmonics; the language of the physicist is inappropriate and misleading in the context of this enterprise. In practice, the language of Aristoxenian harmonics is a fusion of a theoretical terminology drawn from Aristotelian natural science with a sophisticated extension of that used by musicians themselves. It represents the elements and relations proper to music in terms that are meaningful and familiar to those most sensitive to their musically significant nuances.

Platonist and 'Pythagorean' approaches to harmonics contrast with that of Aristoxenus at every step. As I said earlier, the principles governing musical relations are on their view not autonomous, but belong to

mathematics. Relations between pitches are to be conceived quantitatively; in essence they are ratios between numbers. The language in which these relations are to be expressed, for the purposes of scientific harmonics, is not that of musicians, which is incurably imprecise, and is designed to reflect only the impressions of the senses; it is the exact terminology of number-theory. Finally, the proper criterion of correctness in musical relations is not auditory perception but pure mathematical reason. A correct attunement is one formed according to principles which are intelligible to the mind, and whose privileged status in this domain can be explained and justified by reason. The question whether human perception will recognise it as correct or musically acceptable is of little importance, or none.

I have stated these contentions in their most radical form. There was, however, no single, monolithic 'school' of mathematical theorists. Their views varied in detail more, perhaps, than did those of the rival tradition, at least after about 200 BC. By that time (if not earlier) musicologists of an empirical persuasion had apparently come to treat the writings of Aristoxenus as carrying overwhelming authority, whereas no single figure had comparable status on the mathematical side. Doctrines attributed to the semi-legendary Pythagoras were indeed accorded unfailing respect, but too little was known of him for this to determine more than the most general outline of their approach. Plato, too, carried considerable weight, but his authority was not felt as binding by all mathematical theorists; and the relevant passages of his dialogues are in any case enigmatic enough to legitimise a variety of interpretations and extensions of Platonic views.

Nevertheless, these theorists shared a good deal of common ground. Their starting point was the observation that differences in pitch are correlated directly with quantitative differences in certain physical variables, and that the pitch-relations most fundamental to musical structures correspond to strikingly simple ratios between values of the variables involved. The relevant facts are familiar. The notes produced by plucking two lengths of a uniform string, one twice as long as the other, will sound exactly an octave apart, the greater length giving the lower pitch. The ratio exemplified here is thus 2:1. The two other structurally crucial intervals in Greek harmonics are the concords of the perfect fifth and the perfect fourth; and these are found to correspond respectively to the ratios 3:2 and 4:3. And just as a fifth and a fourth together make up an octave, so the product of the ratios 3:2 and 4:3 is 2:1. (That is, if a length of string is increased in the ratio 3:2, and the resulting length increased in the ratio 4:3, the final length will be twice as long as the original.)

It could scarcely be a coincidence that the three simplest and most basic musical relations correspond to this orderly set of the simplest ratios

of whole numbers. From here it was a short step to the hypothesis that musically acceptable relations are so precisely *because* they correspond to numerical ratios of some privileged sort. In that case, of course, it could not be the ratios between lengths of string, as such, that are responsible for this harmoniousness. The ratios must in some sense belong to the pairs of sounds themselves, and sounds are not lengths of string; and melody can be produced by other means, for instance through pipes or by the agency of the human voice. Sounds are produced by any of these agents, however (so it was argued from the time of Plato onwards), through impacts that create movement in the air. A sound's pitch came to be treated, then, as a quantitative feature of this movement. But a further step was necessary. If the ratios accessible to observation in lengths of string and the like are to have any musical significance, there must also be a direct correlation between relative lengths of a string (or of a pipe, or appropriate dimensions of other sound-producing agents) and relative values of the physical variable that constitutes a sound's pitch. Several hypotheses were offered about the nature of this variable property of movement; but most theorists related it in one way or another to degrees of rapidity, greater speeds going with higher pitches and slower ones with lower. On some views, this 'speed' is simply the rate of a sound's transmission through the air, so that a sound of a given pitch, on this hypothesis, travels exactly twice as fast as the sound pitched an octave below it. According to others, the relevant variable is the rapidity with which impacts on the air are repeated in the production of a sound, for instance by the oscillating string of a lyre, so that pitch is associated with the frequency with which the resulting impulses follow one another through the medium. In either case, there was no great difficulty in arguing that changes in the value of the relevant variable must be directly correlated with changes in the dimensions of the sound-producing agent. Longer strings, for example, generate slower and less frequent impacts on the air.

The details of these different opinions need not concern us. If relations between pitches are ratios between numbers that measure the values of some such variable, be it speed of transmission, frequency of impact or anything else, and if musical relations are distinguished from others by the form of the ratios that characterise them, the most obvious challenge to the mathematical theorist is that he identify the ratios corresponding to every acceptable musical interval. But this identification, by itself, would leave crucial questions unanswered. If the musicality of the intervals is due to the mathematical form of their ratios, we need to enquire what 'form' it is that is shared by all these ratios and by them alone, what special mathematical features these ratios possess and others lack. Musical ratios must be 'musical' in virtue of their conformity to

mathematical principles of some special sort. Hence the theorist must seek to identify these principles, as well as the ratios themselves; and he must offer some explanation of their status. He must show why it is the ratios formed in accordance with just these principles that are musically well attuned, while those conforming to other intelligible mathematical principles are not. This, in effect, is the challenge that Plato issues to harmonic scientists in the *Republic* (531c); they must enquire 'which numbers are concordant with one another and which are not, and in each case why'.

'Concordance', on the view sketched in the *Republic*, is a property of relations between numbers, and between pairs of sounds only in so far as they are concrete instantiations of these numerical ratios. It follows at once both that the principles on which such concordance is grounded are principles of mathematics, expressing some special modes of mathematical organisation and coherence, and that the language in which harmonic analyses are properly to be conducted is that of the mathematician. For the committed Platonist, it is also a direct consequence of these reflections that the criteria by which the musical credentials of a relation are to be assessed are exclusively rational; a relation is musical just so long as its ratio conforms to principles whose authority is mathematically intelligible. Then if it can be demonstrated from these rational principles, for example, that the ratio corresponding to a certain relation between pitches is not concordant, or that a certain structure is ill formed, these conclusions must be accepted as true, even if it is agreed on all sides that the relation *sounds* concordant, or that the structure constitutes for the ear a perfectly harmonious pattern of attunement. The aesthetic discriminations of the human ear provide no adequate test of the correctness of musical relations. Harmonics, admittedly, must begin from auditory observations, through which such concepts as concordance, attunement and the rest are first suggested to the mind; and these must at some stage be correlated with visual observations, in operations with strings or analogous devices, in order for us to become aware of the quantitative nature of such relations. But once these points have been established, the study of relations between quantities, simply as such, is a task for the mind alone. Perception has done its job, and has no further part to play.

A rather different attitude can be detected in the writings of some other mathematical writers, notably in those of Plato's contemporary, Archytas. Plato knew his work, and drew on it; the mathematical principles of harmonic structure, as he outlines them in the *Timaeus* (35b-36b), are adopted, with some simplifications, directly from that source. Plato elsewhere accuses Pythagorean theorists of neglecting the pursuit of the rational principles which govern the ratios they attribute to

harmonic relations; they do not seek to discover *why* certain numbers are concordant with one another while others are not (*Republic* 531c). Evidently this charge cannot be made to fit Archytas' case. In another respect, however, Archytas falls squarely within the scope of Plato's criticisms in that passage. The Pythagoreans go wrong, he says, by conceiving their enquiry as a search for 'the numbers in the concords that are heard'. That is, they devote themselves to the task of identifying the ratios exemplified in the audible attunements of real musical practice, rather than to that of discovering those systems of ratios which are well attuned by purely rational standards, whether they are used and appreciated in human music-making or not. Here Plato describes Archytas' project exactly. From the fragmentary remains of his work, and especially from the reports of it recorded by Ptolemy, it becomes quite clear that he understood his task as that of quantifying the attunements regularly used in contemporary musical performance. But it is clear also that he sought to do this in a way that would represent them as conforming, simultaneously, to intelligible mathematical principles. This was not a straightforward project. In identifying the relations between pitches in each species of attunement, we may fairly guess, he was guided in part by his ear, but partly also by mathematically based preconceptions about the patterns into which they ought to fall. Observations are likely to have been 'corrected', to some degree, in the light of what was taken to be mathematically proper. Again, the ways in which Archytan principles apply to the systems he quantifies are fairly complex, at least by comparison with the relations between principles and structures in the purely 'rational' construction of the *Timaeus*. Summarily, while Plato, along with many later writers, would argue that structures which fail to conform to principles in a maximally economical way cannot be genuinely musical, no matter how common and perceptually acceptable their use, Archytas apparently assumes that what is heard as musical is indeed so, or is at least a very close approximation to a genuinely harmonious system; and he sets out to unravel the perhaps quite subtle and complex ways in which the structure of any such system, despite initial appearances, is determined in accordance with intelligible mathematical patterns of organisation. Though Ptolemy criticises Archytas sharply, we shall find that his *Harmonics* is much closer in spirit to him than to Plato; and I shall argue that he draws on the details of Archytas' ideas, or of ideas he believed were his, a good deal more freely than he admits. In general, like Archytas, he understands the project of harmonic science as that of marrying a commitment to the authority of mathematical principles with a healthy respect for the data of perception. He unambiguously rejects the hyperbolic rationalism of other, more nearly

Platonist exponents of mathematical harmonics (who ride, in his text, under the undifferentiated banner of 'Pythagoreanism'); and the non-mathematical stance of the Aristoxenians, on his view, is misconceived from the foundations up.

2 Reason and perception

For some six centuries before Ptolemy, philosophers and scientists had been debating the supposedly competing credentials of reasoning and of perceptual observation as guides in the quest for truth. Controversy continued into his time and beyond, and was as brisk among harmonic scientists as anywhere else. Earlier commentators had occasionally surveyed the battlefield; but there is little evidence that exponents of the science themselves, after the pioneering years of the fourth century BC, had developed their positions in the light of sober consideration of the merits and deficiencies of the warring camps. They seem, on the whole, simply to have taken up entrenched positions on one side or the other of long-standing barricades, and to have dismissed alternative positions out of hand.

Ptolemy is an important exception. He shows himself to be well informed about the debate, and he offers sharp criticisms of extreme views on either side. His own position is designed to incorporate promising insights from any doctrinal repertoire, while avoiding the faults they had previously carried with them, and to fuse them into a new methodological amalgam, more balanced and more adequate to its task. The business of the present chapter is to examine the statements he makes on these issues in the opening pages of the *Harmonics*. Here he is not reviewing the postures of his predecessors, though as we shall see immediately, they are implicitly under fire right from the start. He offers a set of general reflections, without overt reference to alternatives, leading towards what amounts to a procedural manifesto. Its relation to the scientific ideologies which he identifies in previous writings will emerge at a later stage.

At the very beginning of his treatise Ptolemy advertises, if only by implication, his rejection both of an empiricism that finds no place for principles established by reason, and of a rationalism for which the judgements of perception are irrelevant. 'The criteria of correct attunement (*harmonia*) are hearing and reason' (3.3–4). At a minimum, this means that neither faculty can be dispensed with in the scientific enquiry that seeks to identify those 'differences between sounds in respect of height

and depth of pitch' (3.1–2) that belong properly to *harmonia*. If read in the light of such ruminations on the 'criterion of truth' as had long been common in the writings of Epicurean, Stoic and Sceptic philosophers, it might mean a great deal more: not merely that these faculties are the resources on which the scientist must call if his enquiries are to have any chance of cognitive success at all, but that through their proper use he can achieve absolutely infallible knowledge on the matters at issue. The distinctions between the notions of a 'criterion' that each of these interpretations would involve have been admirably explored by other scholars, and I shall not pursue them here.[1] I need only say that I find no trace whatever in Ptolemy of any tendency towards an 'infallibilism' of a Stoic sort, or any other. Confident though he may be, in the end, of the truth of his conclusions, it is not because he has engaged in acts of judgement incapable, in their very nature, of being mistaken, or because he has deployed his twin criteria in ways that give a philosophically unchallengeable guarantee of truth. It is rather because he has pursued his researches as a scientist should, and has subjected his procedures and conclusions to appropriate forms of critical scrutiny. His watchful eye has detected no flaws in his arguments, or in the ways in which he has exploited and intertwined the testimonies of the mind and the ear. His confidence is to that extent justified; but it involves no claim to infallibility.

It is clearly Ptolemy's view, at all events, that reason and perception are not competitors for the scientist's allegiance, as some harmonic theorists – and others – had supposed.[2] Properly understood, they are allies, and the scientist cannot afford to ignore either. But in that case, we need clear guidance on the nature of the contribution to the project that each individually makes, and on the precise manner in which they are to cooperate. Ptolemy broaches these issues immediately. Both are criteria, 'but not in the same way. Hearing is concerned with the matter (*hulē*) and the *pathos*, reason with the form (*eidos*) and the cause (*aition*)' (3.4–5). At first glance, Ptolemy might be taken to mean that the things studied by this science, the relevant 'differences between sounds in respect of height and depth of pitch', contain elements or features of several different sorts, that all of them are included among the objects of the harmonic scientist's investigations, but that not all are accessible to the same human faculty. Some are objects of perception alone, to be grasped empirically, by listening to the phenomena; others can be discerned only by the mind, through

[1] See especially G. Striker, '*Kritērion tēs alētheias*' and 'The problem of the criterion', in Striker (1996), 22–76, 150–165, A. A. Long, 'Ptolemy on the criterion: an epistemology for the practising scientist', in Dillon and Long (1988), 176–207.

[2] See the passages quoted from Ptolemaïs of Cyrene and Didymus in Porph. *Comm.* 22.22–24.6, 25.3–26.9, 27.17–28.26.

reason – that is, as it will turn out, through procedures of a purely mathematical sort. On this reading, what had appeared to others as a conflict between reason and perception is resolved by dividing up the territory between them, distinguishing different parts of it as the proper concern of each to the exclusion of the other. Neither faculty will have any business in the other's province.

This interpretation is altogether too simplistic. The operations of the two faculties, and the matters on which they are competent to pronounce, will become interwoven in complex ways as we proceed. We can make a useful beginning by looking more closely at the items, in Ptolemy's quasi-Aristotelian list, of which each has been said to be the criterion, matter and *pathos* on the one hand, form and cause on the other.

Let us begin with *pathos*, a word that I have so far avoided translating. It has a fairly determinate meaning, but a wide range of different applications. In general, a *pathos* is something that a person or thing passively receives or undergoes, something suffered, not done, imposed on a subject by factors outside its own control. Hence it is often used to refer to an affliction such as pain or disease, or to an emotion, conceived as something stirred up in us irrespective of our own will or choice, by events impinging on us from outside. More broadly, a person's *pathē* may be his experiences, not necessarily ones of a distressing sort. Hence in connection with perception a *pathos* may be the content of a sensory experience, the impression made on a person's consciousness by an external object, through the channels of the senses.

That might, conceivably, be Ptolemy's intention here. Our hearing is competent to judge the character of its own impressions, the modifications induced in its state of awareness by an encounter with something acting on it from outside. This is plainly the sense of related terms in a rather similar passage of his essay *On the Criterion*. 'Sense perception we must employ for information about the affections (*pathēmata*) which it undergoes. It reports the truth about them and gives an honest answer if we confine ourselves to the question of how it has been affected (*peponthen*), whereas it does sometimes make a false report about the nature of the object that has given rise to the affection' (10.2–3 = 15.9–13 Lammert). But this interpretation will give quite inappropriate results at this point in the *Harmonics*. If matter and *pathos* are closely related, as the context indicates, the matter in question must be that on which the relevant *pathē* are imposed; and if the *pathē* are impressions or modifications imposed on the hearing, the matter affected must be that of which the organ or faculty of hearing is itself made up. But Ptolemy tells us that hearing is a competent judge of the matter as well as the *pathos*, and it is plainly not competent to identify the nature of the matter of which its own

organ is made. More generally, the *Harmonics* – perhaps surprisingly – contains no discussion and makes no use of the familiar distinction exploited in this passage of *On the Criterion*; nor does Ptolemy betray the least interest in either the physiology or the psychology of hearing. His focus is consistently on what is there to be observed, through the senses, in the world outside us; and questions about our reliability as judges of the contents of our own awareness, or about its relation to what is externally 'there', simply do not arise. The issue he is tackling here concerns the means by which we can get an accurate grasp on the nature and interrelations of sounds themselves, conceived as events in an external, physical realm, which the faculty of hearing enables us to observe.

Then the 'matter' of which hearing makes us aware must simply be sound. Hearing is competent to inform us that it is in the medium of sound that the relations in which we are interested are instantiated. The *pathē*, correspondingly, are modifications of sound, not of our own organs or faculties, attributes imposed on it by some agency. The word has already been used in a similar way at 3.2, where sound itself is described as a *pathos*, an attribute or modification, of air that has been struck. But the reference at 3.5 cannot be quite the same. If the matter detected by hearing is sound, and sound is a *pathos* of air, which it receives when subjected to an impact, then in 3.5 this *pathos* has become, in its turn, 'matter' for the reception of further *pathē* such as pitch and volume. Sound may indeed be a *pathos* of the air; but we need no instruction in harmonic or acoustic theory to grasp the point that hearing does not perceive it in this guise. It identifies it as the stuff of which all objects in its domain are made, the 'matter' specific to items in the audible realm; and it perceives it as something that can be modified in ways that alter its perceptible attributes. Sound is 'matter' for these attributes in an Aristotelian sense, since they cannot exist except as its qualifications, while sound itself persists as the underlying subject of their changes, remaining the same thing, sound, through all their variations.

Neither the form nor the cause of the phenomena presented to our ears falls within the province of hearing. If sound is a condition of air when it has been struck, the form of a sound must be a specific variant or qualification of that condition, one that distinguishes it from other such conditions that are also sounds. This form might be conceived as standing in either of two relations to the special audible features, the *pathē*, which the ear detects in the particular sound. It might be thought of, first, as their cause, where this term is construed as referring to some agent or event distinct from that which is caused, and responsible for bringing it about. This is Ptolemy's regular use of the word *aition*; and if form stands in this relation to the audible *pathē*, we should probably take the

expression 'the form and the cause' (*to eidos kai to aition*) at 3.5 as a hendiadys, 'the form, that is, the cause'. The repetition of the definite article does not encourage that suggestion, however; nor does the parallelism between this phrase and its predecessor, 'the matter and the *pathos*', since these are plainly two different items. Alternatively, these two things, the formal qualification and the audible feature, are identical with one another. In that case the differences between the descriptions we give of the perceptible features and the formal attributes will not be due to any real distinction between them, since only one thing is being described, but to differences in the manner in which this subject is grasped by hearing and by reason. Ptolemy's initial definition seems to imply that this second interpretation is correct. Sound *is* a condition of the air, rather than being something caused by such a condition; and hence modifications of sound will *be* qualifications of that condition. Since, as will shortly become clear, it is reason and not perception that identifies sound as a condition of this sort, it must be reason also that grasps, as such, its qualifications; and these must be its forms, or elements of form. This diagnosis is confirmed later, in I.3, where Ptolemy argues that differences in the perceptible pitches of sounds *are* differences of quantity (8.15–17), rather than that quantitative differences in the condition of the air are distinct items which stand to them as their causes. This version of the quantitative approach to harmonics is indeed essential to his enterprise, since he will need to represent the ratios (that is, the principal 'forms') that the science studies as ratios between the pitches of the sounds themselves – not merely as ratios between other things that are causally related to those pitches. Hence though the pitch of a sound is not perceived by the ear as a quantitative attribute, capable of entering into formal relations with others, it must nevertheless be one; and it is represented as such by the faculty of reason. The form of a condition of the air is thus the intelligible aspect of something perceived by our hearing as the pitch of a sound.

In that case, the value of reason as the faculty which identifies form cannot be due, or not directly, to its privileged access to the cause of a sound's pitch. Reason is indeed involved also with causes, as we have been told; but when it focusses simply on form, its objects are not causally related to the phenomena that are heard. They are identical with them. In the context of the *Harmonics*, then, one might well ask what is gained by calling on reason in this role, if it directs itself to nothing beyond what hearing has already detected, and merely represents it in a different guise. Ptolemy's next remarks shed some light on this issue.

It is characteristic of the senses to discover what is approximate and to adopt from elsewhere what is accurate, and of reason to adopt from elsewhere what is approximate and to discover what is accurate. For since matter is determined and

> bounded only by form, and modifications (*pathē*) only by the causes of the movements, and since of these the former [i.e. matter and modifications] belong to sense perception, the latter [form and the causes of movement] to reason, it follows naturally that the apprehensions of the senses are determined and bounded by those of reason, first submitting to them the distinctions that they have grasped in rough outline – at least in the case of the things that can be detected through sensation – and being guided by them towards distinctions that are accurate and accepted. (3.5–14)

The senses, then, are insufficiently exact instruments to make the precise discriminations required by the scientist. Reason, on the other hand, is powerless on its own. It has no independent access to the data, but must take from other sources the rough and ready information they give about the contents of the world. (As Ptolemy puts it in *On the Criterion* (13.18–20): 'Mind could not begin to think of anything without a transmission from sense perception.') Its task is then to refine this information. What this refining involves is not yet clear in detail; but we know at least that in transforming representations of *pathē* into analyses of form, we shall be deploying concepts that are much more sharply defined and precisely distinguished from one another than are the categories under which things are differentiated by the ear.

The superior reliability of reason is to be accounted for, Ptolemy continues, by the fact that it is 'simple and unmixed', not affected, that is, by material changes; hence it is 'always the same in relation to the same things'. Perception, by contrast, 'is always involved with multifariously mixed and changeable matter, so that because of the instability of this matter, neither the perception of all people, nor even that of the very same people, remains the same when directed repeatedly to objects in the same condition' (3.13–19). (Similarly at *On the Criterion* 15.1–4: 'Perception [unlike reason] . . . can often be affected by similar things in opposite ways and by opposite things in the same way.') The general sense seems clear. Perception is unreliable because its discriminations are altered by our own bodily changes; and they differ from person to person and from time to time because of variations in people's material composition and condition. The interval constructed by combining a perfect fifth with one that is nearly, but not quite a perfect fourth may be perceived sometimes or by some people as an octave, but not at other times or by others. It all depends on the current material condition of their auditory apparatus. Under no conditions, by contrast, will reason accept that the ratio 2:1 results from the composition of 3:2 with any ratio other than 4:3.

Ptolemy does not pause to explore the metaphysical content of the thesis that reason is 'simple and unmixed'. It had a long and distinguished history in Greek philosophy from the fifth century BC onwards, and was

available, under various interpretations, to Platonists, Aristotelians and Stoics alike. There is no good reason why Ptolemy should examine its credentials, or even identify the exact nuance of interpretation, if any, which he prefers; broad agreement to the proposition could reasonably be assumed. No more does he argue or dissect his general statement that the senses are essentially bound up with diverse and changeable matter. This too was a philosophical commonplace, and to investigate either in depth would evidently take him too far from his intended subject.

Ptolemy's appeal to a fluid material realm in which perception is caught up, but from which reason is separate and detached, seems well adapted to explaining why reason is consistent in its judgements while perception is not. It is less obvious that it can account for their different degrees of precision, since it is perfectly possible for the members of a series of judgements to be inconsistent with one another while each is entirely precise, or for them to be unfailingly consistent but just as unfailingly vague. It would take a number of additional assumptions to close this gap in Ptolemy's account. We might assume, for instance, first that the matter of our sense organs is not only subject to change but actually in a process of change at all times, and secondly that no act of perception is instantaneous. In that case every perceptual representation would inevitably be hazy in its outlines, like a snapshot taken while the camera is wobbling.[3] By contrast, since reason is 'autonomous' (*autotelēs*, 3.15), unaffected by change, there is nothing to blur the precision of its representations. It is not clear, however, that Ptolemy intends to commit himself to so much, or to the further assumption (which also seems to be required) that change is the only factor that can interfere with the precision of any rational or perceptual judgement. The question must be left unresolved.

The consequence that Ptolemy immediately draws from his brief metaphysical excursion seems, at first sight, almost equally vague. Since hearing has an essential role to play in making judgements concerning attunement, but lacks consistency, precision and accuracy, it requires help and instruction from reason, like a stick to lean on (3.19–20). One can hardly quarrel with this remark, or assent to it either, until the nature of this help has been clarified. Some guidance is offered at once. Hearing, like sight, despite its unreliability when used alone, is capable of making an important kind of comparison and evaluating it correctly; that is, it can and standardly will recognise the superior credentials of things constructed according to rational principles, when it is confronted with them and compares them with ones constructed otherwise. Thus we may ini-

[3] Compare e.g. Plato *Theaetetus* 156a-157c, 181b-183b.

tially see a figure drawn freehand, by eye alone, as a perfect circle when in fact it is not; but when our eyes compare it with one drawn to the correct formula with the help of reason, they realise their mistake and recognise the accuracy of the latter. Similarly, a musical interval attuned by ear may at first seem faultless; but when we compare it with one attuned according to the ratio prescribed by reason, our hearing – the very same faculty that made the initial mistake – recognises the 'legitimacy' of the latter and the 'bastardy' of the former (3.20–4.7).

These claims rest on an appeal to experience, as evidently they must, not on philosophical argumentation. So too does Ptolemy's next, more general thesis, that it is always easier to judge something than to produce it, a proposition illustrated by a string of examples (4.7–9). The relevance of this claim is not immediately obvious, but it becomes so later. Ptolemy will insist that harmonic relations constructed on the basis of rational principles are not for that reason to be accepted without question as correct, but must be submitted also to the judgement of the ear. The present thesis is designed to support a superficially puzzling implication of this procedure, that hearing is competent to assess the accuracy of relations which it cannot be trusted to construct. As Ptolemy points out, one can be a good judge of wrestling or dancing or pipe-playing or singing without being a good practitioner of these arts. In the case we are interested in, the ear is much better equipped to judge the relative accuracy of two competitors for the title of 'the perfect fourth' when it is presented with both and compares them, than it is to construct or identify a single one unaided. Taking this thesis together with the previous one, which postulates a situation in which one of the competitors really *is* a perfect fourth, rationally constructed, Ptolemy can conclude that the attentive ear will surely select the right candidate.

Most of the remainder of I.1 is devoted to one further contention and its elaboration. The senses' reliability as unassisted judges of quantitative differences, we are told, becomes less as the difference becomes a smaller fraction of the sizes of the things compared. Mere difference in quantity, as such, is easily discerned. It is harder, but not too difficult, to construct by eye a line that is double or half the length of another, or to recognise the relation when we meet it. Tripling a length or dividing it into thirds, or identifying these relations accurately by sight, is harder still, and so on through increasing levels of difficulty. These remarks are plausible enough; but like several others in this introductory passage their relevance to the subject in hand does not leap to the eye; and Ptolemy does nothing yet to explain it. The issue need not detain us at present. We shall return to it in Chapter 5, where it will turn out to be of quite fundamental importance.

Ptolemy has spoken repeatedly of relations constructed 'rationally', 'by reason', or 'with the help of reason'. So far these notions remain uninterpreted; and they plainly require interpretation, since it is not obvious how reason can construct anything whatever in the perceptual domain, capable of being presented to the judgement of the senses. The puzzle is partially resolved at the end of I.1. Hearing, like sight, needs the help of 'some rational criterion working through appropriate instruments' (5.3–4). It is these instruments that are the key. In the case of sight, they are such things as the ruler, used as an instrument for constructing or assessing straightness, and the compasses, designed to assist in the construction of a circle and the measurement of its parts (5.4–6). Ptolemy does not explain the sense in which these devices are instruments of reason, but the idea behind the description is clear enough. The circumference of a circle, for instance, can be drawn accurately with the aid of compasses, because a circle is 'rationally' defined as a closed plane figure every part of whose circumference is equidistant from a given point; and the compasses are designed to ensure that the curve they describe, when properly used, fits the rational definition. They are not designed merely on the perceptual principle that they should generate a figure that looks round.

Then if the ear is to be presented with sonorous relations constructed, in a parallel sense, on rational principles, some comparable instrument must be found by which we can transfer into the auditory domain the quantitatively defined relations derived from those principles by reason. If, for example, we are led by reasoning to the conclusion that pitches related in the concord of the perfect fourth must stand to one another, from a formal point of view, in the ratio 4:3, we cannot bring this proposition to the judgement of hearing by constructing an interval in this ratio simply by ear, and then attending to its perceptible character. The ear has no way of deciding, by itself, when this ratio has been instantiated, since it does not perceive pitch-relations as ratios between quantities. All it can do is to judge whether the conjunction of a given pair of notes generates in sound the *pathos*, or the relation between *pathē*, which it recognises as the interval of a fourth. It can neither tell what the ratio between their pitches is, in its formal aspect, nor construct unaided any interval on the basis of a description given only in terms of its ratio. We therefore need some technical device, so designed and manipulated that we can be sure that the pitches which it generates in sound are related in the ratio we intend. Then they can be submitted to auditory tests.

At this point we must return briefly to an aspect of the opening discussion that we have so far neglected. It is through reason, Ptolemy said, that we can identify both the forms and the causes of relations between

pitches. These causes are further specified (3.9) as 'the causes of the movements', that is, of the movements of the air that constitute sound, certain of whose variations are the formal counterparts of changes in the sounds' pitch. So far these causes have played no part in the discussion. They become relevant with the introduction of the topic of instruments through whose use formal relations between pitches can be accurately constructed. We cannot build instruments to produce pitches in determinate formal relations unless we know how relations between features of the sound-producing device are correlated with relations between the pitches of the resulting sounds, conceived under their formal aspect. We must therefore understand the means by which sounds of different pitches are caused, and how the alterations that we can deliberately bring about and identify accurately in the causal agent are linked to precisely determinable changes in the pitches produced. On this account, an understanding of such causes is not in itself, at least at this stage, a goal of the harmonic scientist's investigations. It belongs in that sense to the related but distinct science of acoustics. Its role in the harmonic project is quite strictly an instrumental one, and the speculations in theoretical physics which will yield such understanding, and are examined by Ptolemy in I.3, are an essential preliminary to the pursuit of the science rather than a part of it.

At the beginning of I.2 Ptolemy names the instrument which will serve the purpose he has outlined. It is the *kanōn harmonikos*, more familiarly known as the monochord. Its construction and the principles of its use are described later, in I.8, and elaborated elsewhere; we shall consider some aspects of them in Chapter 10. He continues with a resounding statement of the purposes of the science of harmonics, in a passage that deserves to be quoted in full.

The aim of the student of harmonics must be to preserve in all respects the rational postulates (*hupotheseis*) of the *kanōn*, as never in any way conflicting with the perceptions that correspond to most people's estimation, just as the astronomer's aim is to preserve the postulates concerning the movements in the heavens in concord with their carefully observed courses, these postulates themselves having been taken from the rough and ready phenomena, but finding the points of detail as accurately as is possible through reason. For in everything it is the proper task of the theoretical scientist to show that the works of nature are crafted with reason and with an orderly cause, and that nothing is produced by nature at random or just anyhow, especially in its most beautiful constructions, the kinds that belong to the more rational of the senses, sight and hearing. (5.13–24)

At the heart of these important statements is the notion of a *hupothesis*. I have rendered the word as 'postulate'; but as a translation it is hardly adequate. *Hupotheseis* are 'things laid under', the foundations of any

material or intellectual structure. In philosophy and science the word is regularly used of propositions 'laid down' as the starting points from which arguments or investigative procedures begin; they are fundamental propositions which are not formally derived from others that the discipline has already established, but which form the basis for the derivation or explication of subordinate propositions. There are several more specialised uses and some more casual ones; in Aristoxenus, for instance, the best translation of the word *hupothesis* is often merely 'proposition'. It is important to recognise that in none of these uses does the word's application automatically or even usually imply that the proposition is 'hypothetical' in our sense, something that we are trying out, and whose credentials are so far uncertain. That implication may indeed be at work in some well known passages of Plato, for example (notably at *Republic* 510b-511d, though even there the interpretative issues are complex and controversial). Much more commonly, however, a *hupothesis* is a postulate or principle from which argumentative demonstration proceeds, regardless of whether it has at this stage the status of an agreed axiom, a confirmed fact or a mere guess. It is true that an investigator may sometimes adopt a *hupothesis* and then proceed to test it, logically or empirically (as Ptolemy claims to do in this work); but if it is confirmed, and subsequently used as an axiom or principle of the science, it remains a *hupothesis*, a foundation, even though it is no longer 'hypothetical'. Even if it is plainly false, it is still a *hupothesis* (though a mistaken one) so long as it is used as such. This flexible usage is followed by Ptolemy. In the present passage, for example, the *hupotheseis* mentioned are plainly regarded as truths to be defended, whereas those mentioned at 11.7 and 13.2 are ones which, though they are used as principles by certain theorists and therefore merit the title *hupotheseis*, fail essential tests and are false.

I have suggested elsewhere[4] that at certain moments in the *Harmonics* the word *hupothesis* is best construed as referring to principles inherent in the world itself, aspects of the reality 'underlying' the behaviour of perceptible things, rather than merely to propositions about them enunciated by the scientist. This cannot be regarded as certain, though it arguably gives the most plausible reading of 100.25, and perhaps of a handful of other passages too. But there is a related and much more significant point on which I must certainly take a stand. It is that in cases where a *hupothesis* has been established as scientifically reliable, it is so because it is true, because it formulates as a proposition a principle that holds in the external world. It is not just a convenient fiction by which the scientist is enabled to organise his data. The point must be made, if only

[4] See *GMW2*, p. 279 n.15, 380 n.38.

in view of the 'instrumentalist' interpretation of Ptolemy's astronomy that was championed by Duhem, and is still occasionally found in discussions of the *Syntaxis*. So far as astronomy is concerned, the issue should have been laid to rest by the cogent arguments of Alan Musgrave (1981), and I shall not pursue it. The astronomer's *hupothesis* that the stars and planets move in courses that are compounded from perfect circles is intended as a true statement about celestial mechanics, not merely as a mathematical supposition which provides the scientist with a convenient way of analysing, describing and predicting the movements of the heavenly bodies. In the *Harmonics*, fortunately, there is much less room for dispute, since the mathematical *hupotheseis* deployed in its theoretical derivations are explicitly underpinned, in their turn, by the results of investigations in physical acoustics that occupy I.3. Relations between pitches can properly be represented as ratios between quantities because that is what, in physical reality, they are; and if they were not, no amount of mathematical or methodological conjuring would, in Ptolemy's view, justify the *hupotheseis* that treat them in this way. Certain high-level *hupotheseis* about the mathematical character of the ratios that are assigned a privileged place in harmonics are justified similarly, as we shall see in Chapter 5, by their status as accurate representations, in their formal aspect, of the structure of processes actually going on in the physical realm, which in their perceived guise are assigned privileged musical status by the ear. The construction and use of Ptolemy's experimental instruments depends equally on the physical truth, not merely the mathematical convenience of the *hupotheseis* that guide their design. I shall return explicitly to this issue only rarely in later chapters; but unless a great deal of what I have to say there is mistaken, the whole thrust of Ptolemy's method in the *Harmonics* and many of its minutiae will confirm the view that I have stated.

Ptolemy's expression at 5.14, however, 'the rational *hupotheseis* of the *kanōn*', is not altogether clear. The parallel phrase used just below, 'the *hupotheseis* of the movements in the heavens' (5.16), refers plainly to the principles governing those movements, or to expressions of those principles as scientific propositions. By analogy, then, the rational *hupotheseis* of the *kanōn* should be the principles, or statements of the principles, by which that instrument's behaviour is governed. But that suggestion is still ambiguous. The instrument itself is designed on rational principles, and behaves in ways that are governed by them. It is also designed, however, to behave in accordance with rational principles in a different sense, that is, to generate in the medium of sound, and to present audibly to the ears, sets of relations which have themselves been constructed, in thought, by derivation from rational *hupotheseis*. The 'rational *hupotheseis* of the

kanōn' might then be those governing the instrument's design. Alternatively, they might be those drawn on by the scientist when he uses it to construct 'rational' relations, the principles according to which those relations are to be counted as rationally correct. The language of Ptolemy's expression can bear this latter sense only with difficulty; but it is surely required, even though the principles governing the instrument's design are also, in Ptolemy's sense, of a 'rational' sort. The astronomer's task is to find ways of showing that despite the superficially chaotic appearances, the heavenly bodies do move in accordance with intelligible principles, and that the observations, when properly conducted and understood, are 'concordant' with these *hupotheseis* rather than conflicting with them. Correspondingly, the project of the harmonic scientist is not the relatively trivial one of confirming that the principles on which his monochord has been built are correct, though that must indeed be done along the way. It is to show that when appropriate comparisons are made, the ear will accept as musically well formed just those relations which rational principles determine, and which can be offered to the judgement of our hearing through this instrument's operations. The beautiful constructions of nature, as the second sentence quoted above asserts, are not formed randomly, but are crafted according to principles determined by reason and accessible to it. The scientist's task, both in harmonics and in astronomy, is to show that this article of faith is true.

Ptolemy's exhortation, to 'save the *hupotheseis*', is evidently related to the more familiar project of 'saving the phenomena'.[5] The difference is one of perspective and emphasis. In both cases the goal is to show that the truths accessible to reason and the phenomena presented to the senses are in harmony with one another, and that if their evidence is judiciously considered one can consistently accept both. They are parallel representations of the same reality, though one is of course more fundamental than the other. If we conceive the enterprise as that of 'saving the phenomena', we imply that it is the perceptual appearances that are at risk if we fail; that is, if the task proves impossible, it must be because our perceptions are so distorted and unreliable that no interpretation of them can 'save' them as representations of the way things are. That reality is governed by rational principles remains axiomatic. What would have to be abandoned is the supposition that perception gives any worthwhile guidance about its nature and contents.

Ptolemy's formulation here implies the reverse. It is the principles and not the phenomena that are at risk. It is worth pausing to ask what this means – certainly not that the perceived appearances represent reality to

[5] See especially G. E. R. Lloyd (1978).

us exactly as it is, since this, as we have already seen, is a proposition that Ptolemy denies. The core of the matter lies in the peculiar character of the subject matter of harmonics, which is not merely sound, or pitched sound, but exclusively those special relations between pitched sounds that are instances of perfect attunement, recognised by perception as manifestations of musical beauty. (The science's particular focus on beauty is heavily underlined later in the work, in III.3, where Ptolemy looks back at the achievements of his study, and reflects on the relations between harmonics and other sciences; see Chapter 12 below.) The perceptions that he will leave unquestioned, then, are not ones of an evaluatively neutral sort – the perception that this or that note is high-pitched, for instance. The privileged assumption is rather that audibly harmonious interrelations of musical sounds, when perceived as such by the ear, are indeed things of outstanding beauty. It is the excellence of these relations, not the empirical character of their perceived constituents, that cannot be doubted; and it is precisely this excellence, of whose existence the ear assures us, that the scientist sets out to analyse and explain.

Ptolemy nowhere attempts to justify argumentatively his confidence in the reality of this audible beauty. He thought, perhaps, that it is so plainly apparent that to deny or even to doubt it would self-evidently be absurd. A more compelling consideration for sceptical minds might be this. Unless we rely to that extent on the evidence of our hearing, there will be no grounds for supposing that relations grasped as intellectually elegant by the mathematical mind can generate any material counterpart of this elegance when instantiated in the physical realm. Even Pythagorean harmonics in its most abstract mathematical varieties was compelled, as commentators noted, to begin from the observation that certain simple ratios correspond to sonorous relations that are perceived as perfect and musically fundamental by the musical ear.[6] There was nothing else to assure them that these ratios must be the foundations of an order of beauty and excellence.

Ptolemy, like the Pythagoreans, treats perceptual appearances of this special sort as an absolute given. There *is* beauty in the phenomena presented to the senses. The scientist's task is to account for it; and he cannot do this by drawing attention to the perfection of some abstract system of relations that is not instantiated in the phenomena at all. To 'save the *hupotheseis*', in this context at least, is not just to show that certain complexes of mathematical propositions are true, but to show that they are indeed the principles which underlie the facts to be coordinated and explained – that is, that certain perceptible patterns of sound-relations are admirable and beautiful, while others are not.

[6] See e.g. Porph. *Comm.* 23.24–31, 25.25–26.1.

In a programme of this sort it is not possible to call in question the genuineness of these audible excellences. It is possible, however, to question the assumption that they reflect the ordering of physical entities or events according to intelligible principles that are perfectly or 'beautifully' coordinated from the perspective of mathematical reason. In fact, of course, Ptolemy is entirely confident of the truth of this assumption. But confidence and scientific demonstration are quite different things, and if he begins from the former, it is the latter that his treatise sets out to provide.

The direction from which he views the harmonic scientist's task has an important bearing on his method. A Platonist, or a 'Pythagorean' of the sort to whom Ptolemy gives that name, when faced with phenomena that cannot be reconciled with his principles, may adopt the strategy of dismissing the perceptual impressions as distorted and erroneous.[7] For Ptolemy, by contrast, such recalcitrance on the part of the phenomena does not entitle us to reject them; but neither should it lead us to abandon the general hypothesis of rational order. It should induce us, instead, to revise our conceptions, or our applications, of the principles on which that order is based (see especially 6.1–5, 15.3–5). Only so can the rational *hupotheseis* be 'saved', in their role as the formal principles underlying the phenomenon of audible beauty.

This position has an interesting corollary. In criticising certain theorists who go to work on the basis of the assumption that he himself seeks to substantiate, that harmonic relations are governed by rational principles, Ptolemy can be scathing in his comments on the shortcomings of their principles in their allotted role, and of the ways in which these theorists apply them. But he never suggests that the principles such people adopt are not rational in the required sense, or that they are incapable of providing the foundations of any formally well ordered system. They are not principles unacceptable to reason in the abstract; they are merely the wrong ones. Here, then, are further grounds for the belief that reason must take perception as its partner in the enterprise. The correct principles, those genuinely responsible for ordering harmonic relations, cannot be distinguished on purely rational grounds from others that are equally rational, but which happen not to be those operative in the harmonic domain.

Yet the matter can hardly be as simple as the word 'happen' in my last sentence suggests. Ptolemy will argue in III.3–4 that the principles governing harmonic structures are not specific to the field of music, but are those that constitute perfect and beautiful forms of order *wherever* such

[7] See the passages of Porphyry cited in n.6 above, with *Comm.* 26.15–25; cf. 25.10–14.

beauties are to be found in nature – in beings as diverse as the visible heavens, the mind, moral characters and organic bodies, as well as in musical attunements. It seems beyond belief that he supposed that this was a mere accident, something which inexplicably just 'happens' to be so. There must surely be some reason why just these intelligible principles and no others should have this fundamental role in the organisation of admirable and beautiful things so various in nature. Anyone who had reached the view enunciated in III.3–4, and filled out, though sketchily, in the chapters that follow, would be bound to expect that these principles would have some special characteristic, peculiar to themselves, which accounted for their privileged position among all those principles that are acceptable to reason. Since it is reason alone that enables us to recognise these principles and to grasp the formal character of the modes of organisation they determine, one might further expect that the features conferring superiority on the privileged group, and the grounds of their superiority, would themselves be rationally intelligible. Yet Ptolemy identifies no such features, and gives no hint to suggest that there are any. He admits only one method for distinguishing those principles which do govern the forms of harmonic structures and of other beautiful things from those that do not; and it is an empirical one. It is the senses, and nothing else, that will confirm that structures governed by those principles are indeed the ones that perception accepts and admires as perfectly formed. If there is something about the principles as such which distinguishes them intrinsically from others, Ptolemy's silence about it apparently indicates that it is a feature inaccessible to the human mind.

Confirmation of the fitness of any proposed set of principles for the role proposed depends, then, on empirical tests. That task, however, will come in the closing phases of a long investigation. At a much earlier stage the scientist has been faced with the prior project of identifying principles that might plausibly be assigned this position, in a 'hypothetical' way, and whose credentials he will subsequently seek to discover. Unlike some twentieth-century philosophers of science, Ptolemy does not suggest that we should, let alone that we must, initially adopt such 'hypotheses' (in the modern sense) merely by guesswork, inspired or otherwise. In this opening phase of the investigation, he says, the *hupotheseis* 'are taken from the rough and ready phenomena' (5.17–18). The remark indicates that a competent scientist will extract them in some way from observations, that there is some kind of procedure which he needs to follow. The observations themselves are apparently sufficient, at least, to provide legitimate encouragement for the postulation of certain *hupotheseis* and to discourage others. It seems likely that Ptolemy is hinting at procedures of an inductive or abstractive sort. But these procedures must evidently involve

more than simple generalisation from observed data, for at least two reasons. First, observations are in Ptolemy's view inevitably 'rough and ready'; the fine details of the principles must be introduced in another way, by what he describes as the exercise of reason (5.18–19). Secondly, we already know that the organising principles will be expressed in terms of mathematical form, and mere generalisations of the audible data, or abstractions from them, cannot by themselves yield propositions of that kind. The relations in question are not heard, even approximately, as ratios between quantities. The business of extracting *hupotheseis* from the rough and ready data of perception will apparently be a matter of some complexity. I shall not pursue it further here; in Chapter 5 we shall consider the way in which Ptolemy actually approaches the task.

Ptolemy's reflections on the basis and objectives of his science, on the faculties through which its data and principles must be grasped and on the criteria by which its conclusions are to be judged, come together to determine the outline of the method that the harmonic scientist, in his view, is required to adopt. Let us remind ourselves of his task. It is to demonstrate that the systems of attunement which present themselves to the attentive ear as musically beautiful and well formed are so because of their conformity to intelligible mathematical principles. We may distinguish four stages in the investigation which Ptolemy thinks appropriate.

(i) The first is a preliminary set of studies in physical acoustics. Since in perceiving relations between pitches in their character as musical intervals we do not hear them as ratios between quantities, while it is in the guise of such ratios that they are represented in the pronouncements of reason, we must investigate the way in which the perceptible and the formal aspects of these relations are in fact connected. We must also study the relations between quantifiable alterations in features of a sound-producing agent, and the resulting changes in the pitches of sounds produced by its agency. This knowledge will be required at the fourth and final stage of the investigation.

(ii) Secondly, in the light of understanding achieved in the first part of the enquiry, we must formulate hypotheses about the principles which determine the intervals admissible in harmonic systems, and govern their organisation into harmonious patterns of attunement. These principles are mathematical, concerned in the first instance with relations between numbers. It must be shown that they are rationally consistent, and intelligible as determinants of mathematically elegant structures. At the same time, as we have just seen, some evidence must be produced to encourage, though not yet to prove demonstratively, the suggestion that it is relations governed by just these principles that are the formal counterparts of the relations counted as 'beautiful' from a musical perspective.

(iii) Next, we must find, justify and pursue a procedure by which quantitative descriptions of formal relations, and appropriate sets of formal relations, are derived mathematically from the rational *hupotheseis* that have been chosen. It will be necessary to articulate, and again to justify at the rational level, some ways of controlling these derivations, so that the only sets of relations to emerge are ones that plainly correspond, in their general outlines, to the aesthetically accepted contours of musical attunements.

(iv) Finally, we must test the credentials of the relations and systems of relations that have been derived, by submitting them to the judgement of the ear. Here we shall use the monochord, or any of various similar but more complicated devices that Ptolemy describes, to transfer the numerical formulae accurately into the realm of sound. Only if the ear recognises these sonorous counterparts of the systems of formal relations as perfect examples of harmonious attunement will the rational *hupotheseis* governing the latter have been established as the true principles of harmonic order. On these tests hangs the issue of whether the *hupotheseis* have been 'saved', or not.

Set out in this summary way, the procedure looks tolerably straightforward; and it has much to be said in its favour. We should note immediately a feature of Ptolemy's programme which it conceals. His investigation does not run smoothly on through the four stages to its completion, but repeats the sequence (with the exception of step (i)) several times over, as new kinds of relation and new levels of structure are introduced. This is potentially confusing, but in itself creates no theoretical difficulties. There turn out, however, to be quite troubling methodological complications at every stage.

Those involved at stage (i) will concern us in the next chapter. They arise within the science of acoustics, rather than harmonics, and need not yet be reviewed. As to stage (ii), I have already indicated that the initial extraction of *hupotheseis* from perceptual data will not be straightforward; and neither will Ptolemy's account of the *hupotheseis* themselves. Some attention will be paid to these matters in Chapter 4, and they move into the centre of the agenda in Chapter 5. The derivations demanded at stage (iii) become more problematic than might have been expected in at least two respects. First, as I have just indicated, there are several different classes of harmonic relation involved in the structure of any attunement, and there are structures of several kinds and levels. These relations and systems of relations cannot all be derived from the same principles in the same way. The principles must therefore be variously applied, and decisions must be made as to which modes of application are legitimate and appropriate. Secondly, it will emerge that musically recognisable systems are not fully determined by the application of principles that are 'rational'

in the sense that Ptolemy intends, important though they are. Rules of other sorts, based elsewhere than in mathematical reason, must also be applied in the formation of the systems; and there are considerable obscurities about the status they should be assigned, and the relations in which they stand to the pronouncements of reason. Some of these difficulties will make themselves felt in the course of Chapter 7; they are more directly addressed in Chapters 8–9.

The last step, stage (iv), in which theoretically derived systems are presented to the judgement of the ear, will also involve unexpected difficulties. Some of them are technical, concerning the construction, accreditation and use of the experimental instruments (Chapter 10). Others are more vexing from a wider methodological point of view. What the musical ear accepts, as it turns out, will not after all be identical with what reason seems to have determined, but is related to it in a variety of ways, not all of them simple and direct. There are uncertainties, also, about whether Ptolemy aims to demonstrate or test the aesthetic acceptability of all the systems of attunement he discusses, or only some of them, and in that case which ones, and why. It will turn out that a good deal hangs on this issue. For these and other reasons, we shall have to look rather closely at the question whether Ptolemy seriously intended the 'experiments' he describes to be carried out in practice, and to be treated as genuine tests of the credentials of his theoretical conclusions. The fact that he repeatedly advertises them in this role is by itself no proof that he had actually undertaken them himself, or that in his opinion the student of the subject should remain sceptical about Ptolemy's conclusions until he has gone through the experiments, in practice, on his own account. We shall consider these issues in Chapters 10 and 11. Meanwhile we should note that they are important not only for our understanding and evaluation of the *Harmonics* as a work of science, but also for the oblique light that Ptolemy's procedures here may shed on features of his astronomy that have stirred up vigorous scholarly controversy in recent years. If there are good grounds for suspecting that Ptolemy proposes his harmonic 'experiments' in something less than good faith, this will strengthen the hand of those who raise comparable doubts about the methods he adopts in the *Syntaxis*.[8] If we conclude that in the *Harmonics* such suspicions are groundless, that will not of course demonstrate Ptolemy's innocence of the 'frauds' with which his astronomical work has been charged. But it will do something to slant the probabilities in his favour, and all the more so if there turn out to be close parallels between the methods he claims to adopt in each of the two domains.

[8] See especially R. R. Newton (1977).

3 Pitch and quantity

Readers intent on the issues that are central to Ptolemy's methodology can be forgiven for ignoring some of the present chapter's more detailed ruminations in favour of their own, less pernickety reading of 1.3. My remarks in this chapter amount to a partial commentary on that stretch of text, and on little else. As I remarked earlier, the phase of the investigation conducted here, which I labelled as Stage (i), is only a preliminary. It is designed to establish the proposition that pitch is a quantitative attribute of sound, and to identify the causal factors responsible for its variations. Ptolemy treats these questions as closely interconnected. The proposition about pitch cannot be established without a study of the causes; and in practice the two issues are pursued simultaneously. Both have important roles in the sequel. The first will legitimise Ptolemy's policy of expressing pitch relations as ratios of numbers, in accordance with their mathematical forms rather than with the corresponding *pathē*. The second will serve as a basis for correlating the one mode of description with the other; it will also provide an account of the principles underlying the construction of experimental instruments, and the groundwork for an understanding of their use. The main purposes of my project would be served well enough by a bare sketch of the arguments in this passage.

Nevertheless, the details are of some interest, and I shall spend a little time on them. I shall try to show, in particular, how Ptolemy's treatment of the subject, culled though most of its contents are from earlier writings in a well established tradition of acoustic theorising, is accommodated throughout to a particular conception of the relation between causes and effects. That conception, too, is borrowed by Ptolemy from elsewhere, and it is certainly open to criticism; but he adheres to its implications with remarkable, even obsessive, tenacity. We can recognise in this small-scale and relatively self-contained exercise the same generalised passion for methodological consistency as characterises the *Harmonics* as a whole. I shall also draw attention, on the other hand, to various difficulties of detail, anomalies and gaps in Ptolemy's account. Quite apart from issues about the reliability of its 'facts' and the truth of its

theories, it is not in all respects convincing as a specimen of scientific reasoning. By comparison with the remainder of the work, he seems to address the minutiae of physical acoustics with less than fully focussed critical attention.

Differences between sounds, Ptolemy begins, like differences between things of any other sort, are either qualitative or quantitative. He offers no abstract analysis of this distinction, but immediately raises his principal question: into which of these classes do differences in height and depth of pitch fall? (We should notice at once that the commonest Greek words for 'height' and 'depth' in this sense, which are used by Ptolemy throughout most of this chapter and often elsewhere, are as metaphorical as their English counterparts, but that their 'literal' meanings and associations are quite different from those of our corresponding terms. *Oxytēs* is not height, but sharpness, and *barytēs* is heaviness, not lowness or depth. This feature of ordinary Greek usage will have some importance later in Ptolemy's discussion.) The issue, he continues, cannot be settled merely off-hand, but must be approached in the light of an examination of the causes of the phenomena (6.16–18).

This claim is already an interesting one. Differences are quantitative, we shall assume, in so far as they consist in differing values of some variable, such as weight or speed or number. They are in principle measurable (though not necessarily in practice), on some appropriate scale. Qualitative distinctions, for present purposes, are those of all other sorts, and as such they cannot be measured; one cannot even in principle, on the face of it at least, measure the difference between the scent of a lily and the smell of an old kipper. But given the approach suggested by Ptolemy's remark, this 'on the face of it' becomes exceedingly problematic. The question whether a given sort of difference is or is not quantitative is not to be decided by perception, or even by careful reflection on the way in which it presents itself to the senses, but through an investigation into its causes. The implication seems to be that if, and only if, the difference between the causes of certain attributes is quantitative, so too is the difference between the attributes themselves.

In a world like that envisaged by the Greek atomists, this approach might be held to entail that all attributes, and all differences between them, are quantitative. No room would be left for differences to be genuinely qualitative, even if they were perceived as such, since all would depend ultimately on causal factors whose variations are quantitative, in what I take to be Ptolemy's sense. (The difference between the odours mentioned above, for instance, would be causally based on such things as the relative sizes of the atoms emitted from each object, the closeness of their adhesion, the number and acuteness of their angles, and so on.)

Ptolemy, of course, is no atomist; in his more Aristotelian universe it is clear that there are irreducibly non-quantitative distinctions. The remark with which he opens the chapter (6.14–15) and much else along the way unambiguously presuppose that some differences are irreducibly qualitative. All differences whose causes differ quantitatively, however, are themselves quantitative, no matter how they are perceived; and hence the question whether a distinction that seems qualitative actually is so raises quite recondite issues that fall into the province of the scientist. It is not to be answered on the basis of appearances alone.

This brings us back to a position I attributed to Ptolemy in the previous chapter, that the form imposed on some material is not causally related to the corresponding perceptible *pathos*, but is identical with it. In view of the principle apparently operating here, that where the cause of attributes in a range varies quantitatively, so do the attributes, one might think this conclusion unjustified. The proposition that Ptolemy wishes to establish, that the attribute we perceive as pitch varies quantitatively, can evidently be secured without it. If the relevant changes in the original causal agent, the sound-producing device, are quantitative, then by the principle Ptolemy has adopted, the variations they produce in the form of the aerial disturbances will be quantitative too. If these variations in form are conceived as causing, in their turn, variations in the perceptible *pathē* of these disturbances, in their guise as sounds, then these *pathē* too must be quantitative, by the same rule. In that case, forms and perceived attributes will be distinct items, but the fact (if it is one) that differences between forms are quantitative nevertheless guarantees that this is true also of the corresponding differences between the audible attributes.

But this will not do. If the perceptible attributes differ quantitatively, even though this is not how their differences are actually perceived (as it is not), there must be some true description of them which will represent them in a quantitative manner. It will plainly be incumbent on Ptolemy to explain what these descriptions are, and how they are true. No such descriptions are offered, unless they are the descriptions of intelligible form. Whatever properties these descriptions portrayed, the perceptible attributes would have to stand to them in just the same relation as that in which they stand, on my interpretation, to the corresponding forms; that is, they must be identical with them, though they are represented by the senses in a different guise from that in which the scientist describes them. Evidently these additional properties are otiose. The required identity holds between perceived attributes and intelligible forms. Only the mode in which they are represented is different.

Pitch differences, then, will be quantitative if their causes are such as to determine only quantitative changes in the form of the thing affected,

which is sound; and this will be so if the relevant differences in the causes themselves are only of a quantitative kind. In setting out on his investigation of this matter, Ptolemy draws implicitly on his original definition of sound in I.1, 'sound is a *pathos* of air that has been struck (*plēssomenou*)' (3.2). The causes of differences in pitch, he says, 'seem to me to be shared in some way with variations in other sorts of impact (*plēgai*)' (6.18–19). The remark is a little cryptic as it stands, but in the light of the sequel is evidently intended to mean that the causes in question, which are causes of certain kinds of impact on the air, vary in the same ways as do the causes of impacts in general, impacts of any kind whatever.

Ptolemy's enquiry has already taken him beyond the immediate causes of the phenomena with which he is concerned. Their immediate causes must be variations in the impacts themselves, the events that produce the 'modifications' of the air. Ptolemy has gone back a step to causes at one remove, the causes of these impacts. There are good reasons for this. Not only are the characteristics of the agents that cause the impacts more readily subjected to scientific scrutiny than are those of the impacts themselves, but it is those agents, and not the impacts directly, which the experimenter can manipulate on his experimental instrument, as he adjusts the positions of the bridges under the string.

Ptolemy now offers a list of four variable aspects of the factors causally responsible for impacts. Variations of these four sorts will produce changes in the attributes of the resulting event. These attributes, *pathē*, are altered as a consequence of changes (a) in the force applied by the thing that makes the impact (the agent), (b) in the bodily constitution of that which is struck, (c) in the bodily constitution of the agent, and finally (d) in the distance between the thing struck and the origin of the movement. (It is assumed, with Aristotle, that the vigour of a movement fades as the distance from its source increases.)

There follows an argument designed to demonstrate the relevance of the four kinds of variation listed. We know that variations of each sort cause variations in attributes of the resulting event, because 'it is clear that if the other factors involved remain the same, each of the things mentioned, when it is varied in one way or another, has its own specific effect on the *pathos*' (6.22–4). The argument, with its suggestion of a 'method of difference', might be supposed to reflect a prior programme of experimentation, as if Ptolemy had actually tried the experiment of holding three of the factors constant while varying the fourth, in situations involving agents of several kinds. This seems unlikely. It is true that later, when Ptolemy proposes tests for evaluating his harmonic *hupotheseis*, their conditions are meticulously specified; and I shall argue that he conceived them and may even have conducted them as

'experiments' in the modern scientific sense. His discussions of them, especially in 1.8 and 1.11, make it clear that he was vividly aware of the need, in an 'experimental' situation, to be alive to concomitant variations in different factors, and to avoid interference by uncontrolled variables. But in our present passage it is much more probable that his comment is grounded merely in intuitive common sense. By contrast with the discussions of his deliberate test-procedures, he says nothing here about the means by which the extraneous variables might be held constant. Quite casual observation and reflection will have seemed sufficient to confirm that each of his factors can be responsible, independently of the others, for variations in the resulting *pathē*. More generally, we may recall that very little in this chapter is original to Ptolemy. In his *Commentary*, Porphyry finds a host of precedents for his claims, as well as powerful arguments marshalled against theories of this sort by earlier authors.[1] There is little reason to believe that Ptolemy gave the individual issues much independent thought, let alone experimental attention. His contribution (with one possible exception to be noted below) is mainly one of selection and organisation among an existing body of ideas. As we shall also see, he is not always sufficiently circumspect in his choice and arrangement of hypotheses from the pool on which he draws.

Let us now consider the four factors one at a time, in the order in which Ptolemy takes them.

1 The constitution of the thing struck

This first factor is quickly dismissed as irrelevant in the case of sounds. In Ptolemy's treatment, as in that of most of his predecessors from Archytas onwards, the 'thing struck' is always the air. In many cases, of course, sound is caused when one solid body impinges on another, as when a plectrum strikes a string or a stick strikes a metal disk. It is not this impact between solids as such, however, that is held to cause sound, but the resulting impact on the air, made in these cases by the string and by the vibrating surface of the disk. Hence in this usage the string or the disk will be the striker, not the thing struck. In wind instruments, the breath is usually conceived as a missile, propelled down the pipe and causing sound when it strikes the air outside it, through the nearest available aperture (normally a fingerhole). Here too, then, and in all cases, the relevant 'thing struck' is the air.

[1] His discussion of this chapter, which consists very largely of quotations from earlier authorities, runs from 29.27 to 78.2 in Düring's edition. (Ptolemy's chapter itself occupies only three pages.)

But variations in the air's constitution either make no difference at all to the attributes of the sound, Ptolemy asserts, or else those that they make are imperceptible, for the reason that such differences between different samples of air are themselves not accessible to the senses (6.24–7). Most Greek writers tacitly assume that this thesis is substantially true, and at least one earlier theorist explicitly makes a comparable claim.[2] But Ptolemy's reasoning seems curious. We have been given no grounds for the doctrine that imperceptible causes can have no perceptible effects, and Ptolemy, as a scientist, would be exceedingly rash to commit himself to it as a general principle unless good reasons could be found. The thesis it is supposed to support here is of some importance. In his operations with instruments, as I have mentioned, Ptolemy is urgently concerned to ensure that no factor relevant to the determination of pitch remains uncontrolled. Since the constitution of the surrounding air cannot be controlled, or not outside a modern laboratory, it is crucial to establish that it is not a relevant factor. His argument is supposed to show that its variations can have no effects on any of the perceptible *pathē* of sound. If the argument works, or if independently of the argument the thesis is true, then *a fortiori* they can have none on its pitch. (I shall return to the credentials of Ptolemy's argument shortly.)

2 The force exerted by the striker

This second variable, considered at 6.27–7.5, certainly has effects on one of the *pathē* of sound, but not, Ptolemy argues, on its pitch. (The order in which he deals with the four variables, which is not quite the same as that in which they were initially listed, apparently reflects the order of their importance to the subject, starting with the least significant. The first to be discussed has no effects at all; the second has one, but one in which harmonics has no interest; the third has many kinds of effect, some of them affecting pitch; and the fourth is the determinant of pitch with which the harmonic scientist is most closely concerned.) What the force of the striker affects, Ptolemy asserts, is the sound's volume and nothing else. Certainly it does not cause variations in pitch, for when other factors are held constant, we observe no alterations of that sort in a sound, 'but only the greater following upon the more forceful, the smaller upon the weaker' (7.4–5).

Here Ptolemy is concealing awkward problems of at least two sorts. The first is relatively trivial. Among the causal agents whose variations in force he mentions at 7.3–4 are the blowings of a player on a wind

[2] See [Aristotle] *De audibilibus* 800a.

instrument. Several earlier writers had noticed that the same fingering on the *aulos*, a reed-blown pipe, does not always produce the same pitch; changes are introduced by any of several variables, one of which is the 'tension' or vigour of the breath.[3] Anyone who has experimented with a rudimentary reed instrument will know that the pitch can indeed be noticeably sharpened by more forceful blowing, and that sometimes the instrument can even be induced, partly by these means, to jump to a higher harmonic. Ptolemy himself seems at least vaguely aware of such problematic phenomena, when in I.8 he rejects *auloi* as unsuitable instruments for the conduct of experimental tests in harmonics. They are inadequate for the purpose partly because the dimensions primarily responsible for their pitch, that is, their sounding lengths, cannot (on his view) be accurately measured; partly because of 'irregularities' in their structure (probably unevenness in the bore); and partly because of the influence on pitch of variations in the 'blowings-in of breath' (16.32–17.7). When it comes to a practical choice between instruments for the purposes of conducting tests, he is obviously entitled to reject devices that are affected by the rogue factors mentioned here; but in the theoretical context of I.3 he can fairly be criticised for leaving the last of these variables out of account.

That seems a marginal difficulty; but the second is more troublesome. The earliest known writers on acoustics, beginning with Archytas, had tried like Ptolemy to isolate the determinants of pitch from other factors, and to explain what happens to the air on which impacts are made when the attributes of these determinants change. According to the majority of these theorists, pitch alters in accordance with changes in a variable property of the air's movement, a property associated with swiftness and vigour. In Archytas himself, swiftness and vigour are always linked, and no distinction is made, correspondingly, between factors affecting pitch and those affecting volume. Archytas seems to imply that these attributes of sound always vary together. 'Now when things strike our organ of perception, those that come swiftly and powerfully from the impacts appear high-pitched, while those that come slowly and weakly seem to be low-pitched. Thus if someone moves a stick sluggishly and weakly, he will make a low-pitched sound with the impact, but a high-pitched one if he moves it swiftly and strongly. We can grasp the fact not only from this example, but also when we want to utter something loud and high-pitched, either in speaking or in singing, since we utter with a violent breath.' In the sequel, it is in fact the force and vigour rather than the

[3] See especially Aristox. *El. harm.* 42.6–14, and cf. Ar. *De gen. an.* 788a, [Ar.] *Problems* XI.13.

speed of an impact which Archytas seems to hold primarily responsible for a sound's pitch, as well as for its volume.[4]

It might seem appropriate, and would certainly be charitable, to treat Ptolemy as assuming that this problem was simply obsolete. Attempts to modify the Archytan hypothesis, so as to distinguish clearly the determinants of pitch from those of volume, had long ago been made by Plato and by Aristotle; and later writers seem typically to have thought that their contributions had resolved the difficulty. Plato makes pitch dependent wholly on speed, volume on the amount of air moved.[5] Aristotle elaborates the idea. Noting explicitly that voices, or sounds in general, may be simultaneously high-pitched and quiet or loud and deep (a fact inexplicable on Archytas' account), he treats volume as determined by the absolute amount of air moved, pitch by the relation between the amount moved and the force exerted on it, which are jointly responsible for the movement's speed. (Though on Aristotle's view pitch is not the same as speed, it is on a movement's speed that the pitch of its sound depends.) Thus a high-pitched but quiet sound is generated by the movement of a small amount of air struck with considerable force; to make a sound of the same pitch loudly, a larger amount of air must be moved, and still greater force will therefore be needed to move it with the same swiftness. A loud, low-pitched sound involves the movement of a large amount of air under an impact only powerful enough to move it slowly; and so on for other cases.[6]

Whatever the shortcomings of these theories and later variants on them, they at least have the merit of marking a clear-cut distinction between the determinants of pitch and of volume. Ptolemy, however, seems to have fallen back into a position uncomfortably close to the one whose confusions these fourth-century proposals were designed to unravel. He does not of course imagine, as Archytas apparently implied, that pitch and volume necessarily vary together; on the contrary. But his association of volume simply with force and vigour leads him into the old trap. Having denied, in the present passage, that the force of an impact affects the resulting pitch, he adopts a view, later in the chapter, according to which agents with higher 'tension' create higher-pitched sounds, because 'what is tenser is more vigorous in its impacts, the more vigorous is the more compacted, and the more compacted is sharper' (where 'sharper' means 'higher in pitch', 8.4–5). Pitch, on this account, *is* altered by changes in vigour or force; and the same word for 'more vigorous', *sphodroteron*, appears on both occasions, both when he is denying that vigour is capable of affecting pitch and when he is asserting it (7.4, 8.4). We shall consider the details of his theory

[4] Archytas fr. 1 (DK 47B1). For discussion, see Burkert (1972), 379 n.46, Bowen (1982), Huffman (1985 and 1993).

[5] Plato *Timaeus* 67b-c.

[6] Ar. *De gen. an.* 787a, cf. *De anima* 420a-b.

of pitch shortly; meanwhile it is hard to avoid the conclusion that the determinants of pitch and of volume have again become confusingly entangled with one another. It will turn out, in any case, that the routes taken by Plato and Aristotle in distinguishing them were not open to Ptolemy, given the theory of pitch he actually adopts, or at least not in their original form.

3 The material constitution of the striker

Moving on to the third source of variation, Ptolemy first points out that its relevant properties are only those connected with the agent's basic material make-up, the 'primary constitution of its body' (7.6–7). They are listed as density, thickness, roughness and smoothness, and shape, and are contrasted with more 'affective' (*pathētikōterai*) attributes such as smell, taste and colour, which have no bearing on the nature of the impacts that the striker makes (7.7–10). The implication that these are not elements in the 'primary constitution' of a thing's body may suggest that they are conceived as 'secondary' qualities, generated in some way by more fundamental ones. Ptolemy requires no such recondite doctrine, however, only the quite straightforward intuition, confirmable through ordinary observation, that they make no difference to the impacts a thing can make. The colour of an instrument, for example, does not affect any aspect of its sound. He next considers each of the 'primary' bodily attributes in turn, taking them, like the major classes of variable, in reverse order of importance. Two of them, density and thickness, will in fact be discussed together, so that Ptolemy's discussion, and my comments on it, are arranged under only three headings.

3(a) The shape of the striker

The agent's shape – the conformation of the human mouth or tongue, for example – can indeed affect the *pathē* of the sound, but only, Ptolemy tells us, in respect of the sound's own 'shapings' (*schēmatismoi*, 7.12). These account for the differences between 'clatters, thuds, voices, clangs and a thousand other such things' (7.13). A hint of the same idea is given in the Peripatetic treatise *De audibilibus* (800a), an essay which was undoubtedly an important source for Ptolemy in this chapter, as Porphyry indicates. But the notion that a sound has a characteristic 'shape' is hard to interpret, and neither author pauses to clarify it. On Ptolemy's view the resulting qualifications of sound are plainly independent of pitch, and he has no further interest in them.[7]

[7] For passages that do suggest some sort of link between pitch and 'shape', see [Ar.] *Problems* XIX.8, Theophrastus fr. 716 (Fortenbaugh) lines 87ff.

3(b) *The surface texture of the striker*

In connection with the next attribute he considers, Ptolemy alludes to a principle fundamental to the arguments of the *De audibilibus*. Smoothness and roughness, he says, affect 'only a quality in accordance with which sounds are described by the same words, smooth or rough, since the qualities are essentially the same' (7.16–17). The principle behind this statement, which requires the attributes of an event to be the same in character and in name as the attributes of the cause responsible for them, is acknowledged and used pervasively in subsequent passages (e.g. at 7.18–20). The *De audibilibus* offers a formal statement of the principle, as it applies to sounds. 'Whatever character may belong to the sources of the movement of the impacts of the air, the sounds that fall on the hearing will be of the same kind, diffuse or dense, for instance, or soft or hard or thin or thick' (803b). The means by which smooth things generate smooth sounds and rough things rough ones are discussed in the *De audibilibus* several times over (802b, 803b, 804b); Ptolemy sees no need to pursue the matter.

But the principle outlined in this passage is of fundamental importance; it is here that we find the guiding conception of the relation between causes and effects which I mentioned at the start. Though it is spelled out here for the first time, its influence is detectable also in parts of the discussion we have already reviewed. It is this principle, most probably, which leads Ptolemy to the view that we can decide whether an attribute varies quantitatively by discovering whether the cause of its variation is itself a quantitative change. Allegiance to the principle may also be responsible for his too hasty inference that more forceful agents generate only more forceful sounds, not ones of different pitch. Finally, it may go some way towards explaining his apparent adoption of the view that imperceptible changes in causal conditions cannot generate perceptible changes in the effects.

But this would seem a peculiar application of the rule. Perceptibility does not recommend itself as the sort of attribute that can affect a change's causal powers, since it is not an attribute inherent in the change itself, but a function of its relation to our sense organs. On the other hand, if the change in the causal conditions is not merely too slight, but of a kind that is intrinsically undetectable through organs like ours, then, by the principle stated, its effects will be of the same kind, and cannot be perceptible either. The range of this application of the principle would depend on our interpretation of the notion of 'intrinsic undetectability'. It might be attributed to properties and changes that cannot be detected by means currently available to the scientist; or it might be restricted, more

radically, to those that could never be detected, no matter how advanced the technical devices used as aids to observation might be. The former interpretation is unpromising here, since if the changes in question could be detected by means of more sophisticated instruments, there seems no good reason to let the governing principle persuade us that all their effects must be imperceptible in the absence of such devices. Visibility through an ordinary telescope, for instance, seems not to be an attribute different in kind from visibility through the naked eye. (Radio telescopes, electron microscopes and the like raise questions of a different order.) On the second interpretation both causes and effects are guaranteed to be imperceptible. But we could never know, or even have good reason to suppose, that such changes exist; their existence could have no consequences at all for the character of the world as we encounter it, since, by the principle under scrutiny, they could never cause alterations in attributes outside their own, intrinsically undetectable dimension.

3(c) The striker's density and thickness

By the same principle, the third of the attributes of the striker, its density or diffuseness (*puknotēs* and *manotēs*), also creates a corresponding attribute in sounds, making them 'dense' or 'flabby' (*puknous* or *chaunous*). So too does its thickness (Ptolemy's discussion of which is interwoven with that of density, for reasons that will shortly appear): thick things make thick sounds and fine things make thin ones (7.17–20). Though metaphorical, these designations of sound-qualities are reasonably self-explanatory.[8] But both the striker's density and its thickness, Ptolemy continues, affect sounds also in respect of their pitch, making 'heavinesses and sharpnesses' (*barutētas ... kai oxutētas*, 7.20).

Ptolemy's explanation of this fact will depend once again on the principle that effects resemble their causes, but it will be applied in a slightly circuitous way. Heaviness and sharpness, when conceived as attributes of the agent, vary along with the 'kinds of composition mentioned' (that is, thickness and density). Each of the latter depends in turn on the 'quantity of substance' (7.20–21); that shared dependence is what links them together. A denser thing is one having more substance in an equal bulk, and a thing that is thicker than others of equal density has more substance in an equal length (7.22–3). In accordance with the governing principle, one might therefore expect that increased 'heaviness' or depth of pitch

[8] Thickness and thinness are discussed at *De audib.* 803b, 804a. Density is not treated separately, or not under that name. But it is associated with brightness at 801b and 802a, and with thinness at 804a; these qualities, together with clarity, 801b, evidently have a good deal of overlap.

would be associated both with a thicker agent and with a denser one, each of which, in its own way, is heavier; but that is not Ptolemy's intention. Thinner agents create 'sharper' sounds, but so do denser ones, and 'heavier' sounds are produced by things that are thicker, or more diffuse in their material composition (7.23–5). The analogy between the characters of these causes and effects is clear in one aspect of the case of thickness. 'In all other things too, the sharper is described as such because it is thinner, just as is the blunter because it is thicker' (7.25–7). Thinner things are (literally) sharper, and therefore generate 'sharper' sounds.[9] The analogy between bluntness and 'heaviness' in its acoustic sense is intelligible but rather less direct – low-pitched sounds are not described in Greek as 'blunt'; and in the case of density the analogy would apparently hold between the denser and the heavier, which is the wrong way round. Some more intricate explanation will be needed.

The explanation begins at 7.27–8. 'For finer things strike more compactly because they penetrate more quickly, and denser things because they penetrate further.' The sentence serves to remind us that we are thinking about attributes of impacts, and of the *pathē* they impose on the thing struck. But what, we may wonder, has greater 'compactness' of impacts (if that is what *athrousteron* means here) to do with the matter? Ptolemy does not elucidate the point immediately, but offers a series of examples. Bronze makes a 'sharper' sound than wood, or gut-string than flax, because in each case the former is denser. So too, if other features are the same, will a thinner piece of bronze or a thinner string, and similarly for other sorts of agent (7.29–8.2) Then he comes to the point. 'In such cases this happens not through the density or fineness as such, but through the greater tension (*to eutonon*, the property of being 'well-tensed'), since it is an attribute of things like this that they are tenser, while what is tenser is more vigorous in its impacts; the more vigorous is more compacted, and the more compacted is sharper' (8.2–5).

We were told earlier that density and fineness in the agent do indeed attach attributes with the same names and characters to the resulting sounds (7.18–20). As determinants of pitch, however, they function not as causative properties in their own right, but as manifestations of a more inclusive property, tension (though Ptolemy nowhere explains precisely how it is that tension is related to density and to thinness). In the next part of the sentence quoted there are, I think, some elisions in the argument. What is tenser is 'more vigorous in its impacts', no doubt; but when he goes on 'the more vigorous is more compacted, and the more compacted

[9] For a discussion of the influence of these metaphors on scientific analysis, see Barker, 'Words for sounds' (forthcoming).

is sharper' (literally, 'and this is more compacted, and this is sharper'), the focus of thought seems implicitly to have shifted from the agent and its behaviour to the thing struck. Fully unpacked, I suggest, the idea is that because the agent is tenser it is more vigorous, so that its impacts are more vigorous too; the more vigorous impacts are more concentrated, and hence the thing they impinge on is more compacted; and because it is more compacted it is 'sharper', a word intended here primarily, but perhaps not wholly, in its acoustic sense. If this is approximately right, the line of thought from a tenser agent to a more compacted recipient of the impacts is reasonably clear.

We have already been offered a hint of the relation between compactness and high pitch in the thing struck. Density or compactness goes with sharpness, in the non-acoustic sense of the word, because in a denser thing the same amount of substance is compressed into a smaller bulk; and if a thing composed of that much substance in a given length is more closely compacted, it will be thinner and therefore sharper (7.20–3). Given Ptolemy's continual reliance on alleged connections between named properties of the agent and similarly named properties of the resulting sound, we should expect the same relation to hold between density and sharpness in the sound as holds between them in the agent. A fuller explanation would be useful, however; and if the general principle linking the attributes of causes with homonymous attributes in the effects is still to be followed, we also need to be told how the higher pitch of the sound is associated with some form of tension, since it is because they are linked with tension, as Ptolemy has emphasised, that density and thinness are causal attributes of the appropriate sort.

One sentence is offered, after some intervening remarks to which we shall return, to provide the enlightenment we are looking for. 'For sound is a sort of continuous tensing of the air, penetrating to the outer air from the air that immediately surrounds the things making the impacts; and for this reason, to whatever degree each of the things making the impacts is tenser, the sound is smaller and sharper to the same degree' (8.12–15). The first point to notice here is an omission. Ptolemy has not after all quite explicitly identified a sound's pitch with its degree of tension, so making the required link with the tension of the agent. He may have thought it unnecessary to state the identity directly, since it was clearly embedded in Greek linguistic usage. Though it was less straightforwardly colloquial than calling high pitch 'sharpness', it was almost as common, especially in more formal contexts, to refer to a high sound as 'tense' (*eutonos* or *syntonos*). The standard word for 'pitch' and for 'tension' is the same, *tasis* (sometimes *tonos*), and in later chapters Ptolemy uses these words and their cognates to apply indiscriminately to the tension of a

material thing and the pitch of a sound. Greater tension in the agent makes for higher pitch in the sound, then, because both are degrees of *tasis*.

Secondly, the general theory of sound and pitch that Ptolemy seems to adopt here has unusual features. Most Greek theories on the subject identified sound with a movement or movements of the air, caused by impacts. Commonly, though not always, as we have seen, higher pitch was linked with greater speed of movement. Not all authors thought of this movement as the actual transmission of a parcel or stream of air from one place to another. Some authors have the rather subtler conception of air as a stationary or nearly stationary medium, through which no bodily 'missile' is projected, but movement is transmitted rather in the manner of a vibration; there were several variants on such ideas.[10] But movement is an essential ingredient in all these accounts.

Yet with the possible exception of one sentence late in the *Harmonics* (94.25), Ptolemy nowhere alludes directly to an identity between sound and a form of movement. In our present passage, something is certainly transmitted progressively through the air from the source of sound; but it is not said to be a vibration, and still less is the air conceived as moving in place. What is transmitted is a state of tension, which constitutes the sound; by compressing the air on which it impinges, the impact 'stiffens' it, and this stiffening or compacting is heard by us as sound, higher in pitch as the tension is increased. What is perceived is then either the state of tension, or perhaps the event of the air's becoming tense. (In that case the apparent continuity of a sound would be explained by the rapidity with which blows on the air, from a string, for example, succeed one another and repeatedly 'tense' it, so that we do not detect the gaps between them.)[11]

It would be possible to offer Ptolemy a rather different theory, according to which a sound and its pitch are constituted by a vibratory movement in the medium, whose character is altered by the degree of tension imposed on the vibrating air by the agent. But I can see no evidence that this is what he means; movement seems to have lost its traditional role in the analysis altogether. If this interpretation is correct, it marks the only point in his exposition of these matters at which Ptolemy significantly departs from ideas current in the regular Academic and Peripatetic repertoires. The difference would help to explain why he cannot directly adopt their account of the distinction between the determinants of pitch and those of volume. The main advantage of a 'theory of tension', from his

[10] See for instance [Ar.] *Problems* XI.6, *De audib.* 800a, [Euclid] *Sect. can.* 148.
[11] Cf. a passage from a certain Heraclides, quoted at Porph. *Comm.* 30.14–31.21.

point of view, will have been that it allowed him to adhere firmly to the principle on which so much of the chapter's reasoning is based, that effects share their attributes with their causes. Though a theory based on the supposition that swifter impacts create swifter movements does preserve that principle at one level, in its application to pitch it abandons it at another, since there is nothing in Greek ways of representing these phenomena to provide a conceptual link amounting to identity between swiftness and high pitch. They are not linguistically represented as instantiations of the same attribute (though theorists might, and did, argue that in fact they are so); the Greek language does not allow 'swift sound' to mean the same as 'high-pitched sound'. But in representing high pitch as *eutonia*, high tension, the language provides just the conceptualisation that Ptolemy requires. The relevant attributes of cause and effect are the same. We may regard the principle lying behind this strategy as suspect, but Ptolemy, here as elsewhere, at least has the merit of applying its demands consistently.

4 The distance of the thing struck from the source of movement

Ptolemy now states his general conclusion, that difference in pitch is a form of difference in quantity (8.15–17); and we have learned that the quantitative variable in question is tension. With these broad conclusions established, he turns to the last phase of his discussion. So far as the argument of 1.3 is concerned, it deals merely with the fourth of the variable factors to be found among a sound's causal antecedents, the distance between the point from which the striker's movement begins and the air upon which the impact falls. But its conclusions are very important in the sequel. The supposedly direct correlation between relative values of this variable, and relative pitches of the resulting sounds, is fundamental to the rest of Ptolemy's enterprise, since it underpins both the empirical observations from which his theoretical *hupotheseis* are abstracted, and the procedures by which they are ultimately tested. Though features of the physical processes involved are left unexplained and will turn out to pose tricky problems, the gist of the passage is tolerably straightforward.

'Sharpness [high pitch],' Ptolemy says, 'follows upon the smaller distances because of the vigour arising from proximity, heaviness [low pitch] upon the greater because of the relaxation that goes with being further away, so that the sounds are modified in the opposite way to the distances. For as the greater distance from the origin is to the less, so is the sound from the smaller distance to that from the greater, just as with weights, as the greater distance of the weight is to the lesser, so is the downward

thrust from the lesser distance to that from the greater' (8.19–25). Here the focus of the analogy is on the properties of bodies that balance one another at different distances from the fulcrum; the weight, the 'downward thrust' of that which is nearer the fulcrum must be greater than the other's in the same proportion as its distance from the fulcrum is less.

As I remarked earlier, Ptolemy evidently assumes that the force or vigour of an impact diminishes in direct proportion to increases in its distance from the source of movement. Thus, for instance, a stone will strike an object ten feet from the hand that throws it with a force twice as great as that with which it would have struck an object twenty feet away. The rule is evidently false, but that fact need not concern us. We should notice in passing that the 'vigour' (*to sphodron*) associated with proximity is contrasted with the 'relaxation' (*eklusis*) of an impact more distant from its source; the central concept of tension is still at work. When Ptolemy speaks of the converse relation between relative distance and relative pitch, then, the quantitative variable constituting the latter is not simply pitch as it is perceived by the hearing (since it is not readily conceived as quantitatively measurable under this aspect), but the degree of tension in the air, which is not heard as such, but is identical with that which is heard as pitch. The air's tension, of course, is itself not directly measurable, or not with resources available to Ptolemy; but it evidently is something that we must think of as varying quantitatively, and hence as measurable in principle.

What can in practice be measured are the corresponding distances between relevant points on the apparatus causing the sounds. The task of the speculations offered here is to explain why it is that these measurements of relative distance are a reliable guide to relations between aerial tensions, and hence between pitches. They provide an intellectual foundation for the technical operations. If, then, we accept the explanation, we can use our experimental devices, in the first instance, to discover which quantitative relations between degrees of aerial tension are correlated with which perceived musical intervals. The theory allows us to infer, for instance, that if the pitch sounded by a given length of string is an octave higher than the one sounded when the length is doubled, the tension of the former pitch is twice that of the other. Since pitch is *identified* with the degree of tension imposed on the air, not merely treated as its effect, we can now legitimately regard the pitches themselves as standing to one another in the ratio 2:1. We have a quantitative way of representing pitch-relations themselves, not just relations between values of some associated variable. The higher pitch, in this scheme of measurement, will be assigned the larger number. In Ptolemy's later discussions and his tables of harmonic relations, the larger number is in fact regularly assigned to

the lower pitch; but this is simply because he is thinking there, for convenience, in terms of 'distances', lengths of string on his experimental instruments. In the pitches themselves, the relevant ratios will be reversed.[12]

Ptolemy elaborates his account of the relation between pitch and distance, not by further explanations of the physics involved, but with a series of illustrative examples. Longer pipes or strings produce lower pitches, for instance; and he offers an ingenious analysis, consistent with his general thesis, of the workings of the human voice when it moves from pitch to pitch in singing (8.25–9.15). But he seems to have overlooked a real difficulty in the example that is the most important for his subsequent procedures. In the case of pipes and of the human vocal apparatus, he has at least a workable account of what constitutes the distance from the source of movement to the thing struck. In pipes it is the distance from the mouthpiece, where the movement causing the impact originates, to the air outside the pipe at the first open finger-hole. The impulse of breath travels over this distance before striking the air. In vocalisation the thing struck is the air immediately outside the mouth; the other end of the relevant distance is a point on the windpipe from which we contrive to make our breath bounce, on its way to the outside. 'In windpipes [by contrast with wind instruments] . . . the location of the thing struck remains constant, while that of the striker shifts closer to or further from the thing struck, as our ruling principles, with their inborn music, find and grasp marvellously and easily, in the manner of a bridge, the places on the windpipe from which the distances to the outside air will produce differences of sounds in proportion to the amounts by which the distances exceed one another' (9.9–15). This is perhaps bizarre as an explanation, but at least it fits his model.

He offers no such account, however, in the crucial case of strings. Here the relevant distance is that between the bridges determining the string's sounding length. But it is hard to see how this distance can be thought of as constituting the distance between the striker and the thing struck. The string is normally plucked more or less in the middle. It is not sounded by tapping one of the bridges and causing the impulse to strike the air adjacent to the other, as the analogy in the passage just quoted might suggest.

[12] Almost all Greek writers share the view that when one considers the variable whose values constitute pitches, greater numerical values belong properly to higher pitches. When the relation between a higher and a lower note is measured by reference to the ratio between the lengths of string that emit them, the larger number is attached to the longer length, which gives the lower note. Hence the terms of the ratio proper to the pitches themselves is reversed. The only notable and explicit dissenter is Adrastus, who contends, in an extraordinary argument recorded by Theon of Smyrna (65.10ff.), that despite the considerations that the theorists regularly adduce, it is to the lower of two notes that the greater number should correctly be assigned.

It is the string's lateral oscillations, not some postulated impulse along its length, that were regularly conceived as making impacts on the air. Assuming that the source of the impact is located either at the midpoint of the oscillation, the string's position of rest, or at either of the limits of the oscillation, the distance between striker and thing struck will be very much less than the length of the string; and Ptolemy can hardly have supposed that the two distances are directly correlated. In fact no attempt to identify the amplitude of the oscillation as the dimension responsible for a note's pitch will survive a moment's observation, since once a note has been sounded the amplitude progressively diminishes, but the pitch remains constant. Yet he need have had no special difficulty in explaining why a shorter string sounds a higher pitch. Its greater tension, displayed in its lesser flexibility, could (like hardness or density of constitution) be held responsible for its greater vigour in striking the air, and hence for a higher degree of tension in the air itself. By treating string length as a variable with the same causal character as length of pipe, he seems to have made its mode of operation unintelligible. It appears that he has not, after all, provided an account of the relation between distance and pitch which could even plausibly be held to underpin the crucial operations on the monochord. We can offer him one that would serve the purpose, but his own seems strangely unsatisfactory.

Let us return, finally, to a short passage whose consideration we postponed. It comes immediately after Ptolemy's revelation that it is the tension inherent in density and thickness, not density or thinness as such, that is responsible for higher pitch. He goes on:

> Hence if a thing is tenser in some other way, for instance by being harder to a greater degree than it is larger overall, it makes a sharper sound; and where there exists in both of two things something that has the same effect, victory goes to the excess of the one ratio over the other – as when bronze makes a sharper sound than lead, since it is harder than lead in a greater degree than lead is denser than it. And again, any larger and thicker piece of bronze makes a sharper sound than the smaller and finer, whenever the ratio in respect of magnitude is greater than that in respect of thickness. (8.5–12)

The details of this passage pose a number of interpretative puzzles, but I shall pass them by in favour of a straightforward point more relevant to our enquiry. In discussing the ratios between values of the variables he considers, Ptolemy must presuppose that these values are measurable. Largeness (whether of volume or of surface area) and thickness present no general problems; neither does density, for anyone familiar with Archimedes' bath-time discovery. Hardness is another matter. Though we can intuitively agree that its variations are quantitative, I do not think

that Ptolemy had any scale or any instrumental means for measuring them. Hence he has no way of confirming empirically the claims he makes about the ratios of hardness to largeness and to density.

A very similar issue arises in 1.11, where Ptolemy is discussing the use of an eight-stringed instrument to test a proposition. It is essential, first, to confirm that those of the strings' attributes that affect their pitches are so related as to give identical results in all the strings. Ptolemy's procedure depends on an argument to the effect that each of the attributes relevant here, length, thickness and tension, can compensate directly for one another, if the strings are in other respects the same. Two strings of the same length and material constitution, for instance, but differing in thickness and tension, will sound the same pitch if the thicker is also tenser, and if the ratio of its thickness to that of the other string is the same as the ratio of their tensions.

The issue is not whether this thesis is true. It arises from the fact that despite its 'in principle' measurability, Ptolemy is most unlikely to have possessed a sufficiently refined method of measurement to give accurate values for a string's thickness; and the situation in the case of tension is even worse. The only means likely to have been available to him would have been one that applied tension to the strings by attaching to them weights of different sizes. The temptation then is to treat the weights as giving direct measures of tension.[13] This, however, will give the wrong results, since in fact the pitch-ratios will not be correlated directly with those of the weights, but with those of their square roots. We cannot credit Ptolemy with knowing that fact, and merely failing to mention it. A passage in 1.8 (17.7–16) leaves us in no doubt that while he was properly suspicious of 'experiments' with weights, it was for quite different reasons. It rather clearly implies that the ratios of the weights *would* correspond to those of the pitches if certain technical problems could be overcome, so as to eliminate interfering factors. The difficulties he identifies are only practical ones, to do with distortions imposed by the apparatus on the variables measured. They are sufficient to assure us, however, that he did not rely on this way of measuring tensions in experiments of his own; and no others appear to have been available.

I conclude that Ptolemy made no actual attempt to make precise measurements of hardness in the context of 1.3 or of thickness and tension in that of 1.11. A modest amount of observation was enough to show that variations in these factors do affect pitch. But from that point on, we are in the realm of unsubstantiated theory. The precise propositions about

[13] See for instance the tale of the harmonious blacksmith in Nicomachus *Ench.* 6, repeated in Iamblichus, *Vit. Pyth.* 115–19.

ratios involving these variables have no adequate observational basis. Their role is to provide an intelligible framework within which facts crucial to Ptolemy's procedure make sense – in particular, the fact that under certain conditions the ratios between lengths of strings give an accurate measure of the ratios between the pitches they sound. The required conditions can be produced without recourse to these detailed hypotheses about thickness, tension and hardness; the hypotheses give us only a way of understanding what we have done when we have produced the conditions in question. Ptolemy might argue, rather in the manner of the *Syntaxis*, that their truth is confirmed by their 'concordance', and that of their remoter consequences, with the data of perception. But this will not do. Since none of these variables can be measured, the hypothesis that their ratios are related in specific ways *has* no empirical consequences. The available phenomena are equally consistent with any other guess about the details of such relations, demanding only that greater hardness and greater tension raise pitch, and that greater thickness lowers it.

In this chapter I have tried to give some account of Ptolemy's strategy in establishing the preliminary propositions he needs, to draw attention to various intriguing features of it, and to identify some of the difficulties it meets. We may fairly conclude that his record here is patchy. There are few signs that he has undertaken original empirical research; he is content, in the main, to rely on casual observations or on 'facts' reported by his predecessors. The principle governing his discussions of causal relations is again not of his own devising. On the other hand, he applies it with remarkable consistency and some ingenuity, even to the point where it entices him into inconsistencies of other sorts (as in his statements about the relation of 'vigour' to volume and to pitch). His theory of pitch itself, and of the nature of sound as tension, is certainly unusual and may be his own; but whatever its source his adoption of it seems again to reflect his determination to hold relentlessly to the overall causal hypothesis. The discussion as a whole is designed not merely to justify, in a general way, his quantitative treatment of pitch, but to underwrite in detail the operations he later conducts on the monochord, and the conclusions he draws from them. If everything he says were accepted, his account would be well suited to these roles. But we have found weaknesses in certain important areas. In particular, he gives no adequate explanation of the inverse relation between a string's sounding length and its pitch; and he relies on quite unsubstantiated assumptions about the effects of the different variables when combined according to specified ratios. His overall treatment of the subject is at least as good, however, as any other extended Greek essay in physical acoustics, which cannot be held up as a field in which Greek scientists excelled. It is argued out more

cogently and consistently than any of its surviving rivals, and it is admirably accommodated to his purposes. But given the remarkable standards of rigour that Ptolemy sets (or so I shall argue) elsewhere in the *Harmonics*, it must be conceded that this phase of the work does not show him at his best.

4 The ratios of the concords: (1) the Pythagoreans

Ptolemy mentions by name rather few of his predecessors. When he does, it is seldom to record his debts, though some of them emerge clearly enough, as we shall see. His first major topic is the musical concords; and he sets out the approaches of two schools of thought on this matter in some detail, mainly to criticise them. The strategy is designed to throw into sharper relief his own procedures and their merits, and his treatment of the issues is linked very closely to his criticisms of theirs. But the roles of the two critiques in his wider enterprise are different. Only one of them will be discussed in this chapter. (For the other, see Chapter 6.)

In considering what Ptolemy says about these earlier theorists, one of my aims is similar to his own. A study of his criticisms will clarify the challenges which his own procedures must meet, and will provide a yardstick by which we can judge their success from his point of view. But at the same time I shall draw attention to ways in which some of Ptolemy's own views turn out to be developments or refinements of ones he criticises, though he is never quite explicit in acknowledging the fact. His borrowings are worth mentioning not merely to elucidate his intellectual biography,[1] or in the spirit of Porphyry's *Commentary*, to convict Ptolemy of surreptitious plagiarism. In picking out those aspects of his predecessors' work which he chose to preserve, I hope to cast more light on the attitudes and angles of approach that underlie his own.

The principal aim of Ptolemy's discussion of the concords is to show that their ratios can be derived from persuasive rational *hupotheseis*. That is, he will offer an account, grounded in theoretical or 'rational' consider-

[1] This semi-biographical perspective has its own interest, however. If some of Ptolemy's central ideas came from his reflections on earlier sources, then the account he gives, for instance, of the way in which rational *hupotheseis* are to be drawn from perceptual experience may be entirely misleading, if construed as a description of the route by which he arrived at them himself. It is rather a 'rationalised' reconstruction of an ideal procedure. If his *hupotheseis* do indeed stand in the appropriate relation to perceptual data, so that they could in principle have been 'drawn from' them by abstractive methods, this fact will explain his approval and adoption of them, once he has found them. But it says nothing about the manner in which he came to think of them in the first place.

ations, of the general characteristics that ratios must display, the principles to which they must conform, if they are to be the formal counterparts of perceptibly concordant intervals; and he will demonstrate that the particular values of these ratios follow from these principles or *hupotheseis* by logical reasoning alone. Hence the *hupotheseis* concerning the formal nature of concordant ratios will serve to explain why it is that the concords must have just the ratios they do.

An exposition and critique of the treatment of concords by those of his predecessors whom he calls 'Pythagoreans' (I.5–6) prefaces Ptolemy's account (I.7) of his own *hupotheseis* on this topic, and the results derived from them. In I.8 he goes on to describe a way of subjecting these conclusions to empirical tests – the conclusions, that is, that identify the ratios of the concords. This involves for the first time the use of the monochord. Ptolemy adheres very closely, in this phase of his work, to the steps of the programme which he has said scientific harmonics must follow (first the specification of appropriate *hupotheseis*, then the derivation of their consequences, and finally the assessment of these consequences through empirical tests); but at the same time these particular tests may seem unnecessary. Every theorist committed to the expression of intervals as numerical ratios agreed on the values to be assigned to the ratios of the concords. But the move serves several useful purposes. In the first place it completes, in miniature, a simple exemplification of Ptolemy's general procedure, enabling us to grasp its outlines more clearly. Secondly, it gives the opportunity for a discussion of the construction, credentials and uses of the monochord itself. Finally, it provides the basis for Ptolemy's subsequent criticisms (I.9–11) of the propositions about concordant intervals made by adherents to the other major tradition in harmonics, the Aristoxenians. These theorists rejected out of hand the practice of representing intervals as ratios of numbers, and Ptolemy will spend some time in exposing the alleged follies of their alternative approach. But they also rejected the claims made by mathematical theorists for the authority of 'rational' principles, resting their conclusions instead on the evidence of perception. Their conclusions about concordant intervals differ in detail from those of Ptolemy and the mathematical tradition; and by presenting his results not merely as rationally derived (which would not have impressed a committed Aristoxenian) but as confirmed by the most rigorous empirical tests, Ptolemy is able to carry his argument into the enemy's camp, and to show that they are refuted by evidence of precisely the sort that they are officially committed to accepting.

Taken overall, then, his discussion of this topic has three phases. He offers, first, an account and critique of Pythagorean approaches; next he develops his own position, partly by careful adaptations of theirs, expounds their consequences and submits them to empirical testing;

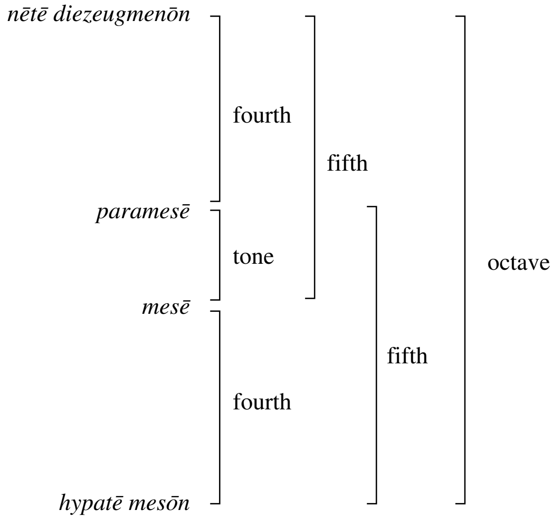


Fig. 4.01

finally he draws on the results established by these means to mount an attack on the presuppositions, methods and conclusions of the most influential school of non-mathematical harmonic theorists.

Before examining the first phase of this programme in detail, I should explain why the project of harmonics, conceived as the study of acceptable forms of attunement and the principles governing them, finds it natural to begin here, with an investigation of concordant intervals. The primary concords (*sumphōniai*), on the Greek understanding of this conception, are the perfect fourth and the perfect fifth, and the interval compounded from them, the octave. There were other recognised concords larger than the octave; but no interval smaller than the fourth, and none intermediate between these three intervals, was counted as concordant. We shall glance briefly, below, at some of the perceptible attributes which were held to distinguish these intervals from all others. But the main reason why it was found appropriate, by theorists of every description, to begin with a study of these relations is quite simple. All systems of attunement used by Greek musicians – or at any rate all those discussed at all closely by the theorists – were in a certain sense 'framed' by notes standing in concordant relations to one another. This arrangement of concords provided each system with its most basic structural features.

In the simplest kinds of system, of which others were conceived as reduplications, transformations or variants, the extreme notes of the

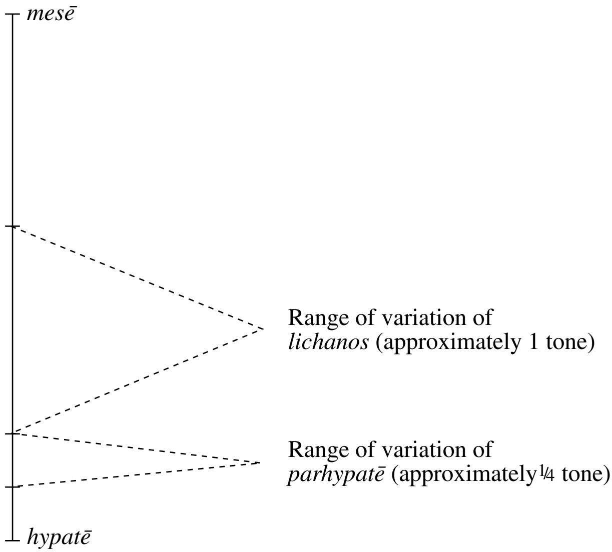


Fig. 4.02

attunement are an octave apart. Its major points of internal articulation are occupied by notes a fourth below the higher limit and a fourth above the lower. Since the octave is the sum of a fourth and a fifth, the higher of these two inner notes is a fifth above the lowest note of the system, and the lower a fifth below the highest. The interval left between the inner notes, which is not of course a concord, was called the tone, *tonos* or *toniaion diastēma*, and its size was regularly defined by its relation to the fourth and the fifth. It is the difference between them, as will be clear from the diagram of the structure given in Figure 4.01. I have added the names by which the notes of this system are usually known.

The four notes of this framework were described as 'fixed' or 'standing' notes, since the relations between them do not change. When we move on beyond a consideration of concords, the system will be completed by the division of each of the fourths into three intervals, that is, by the insertion of two more notes between its boundaries. The system therefore consists, as a whole, of two groups of four notes each, two 'tetrachords', each spanning the interval of a fourth, separated or 'disjoined' by a tone. These new notes, those internal to the tetrachord, were variable in position, and were therefore described as 'moveable'. Different relations between them, and between each of them and the tetrachord's boundaries, defined different systems of attunement (see Figure 4.02).

From the theorists' point of view, the tetrachords in any one system of attunement were identical in form. It follows that each note in a tetrachord of such a system stands in a concordant relation to its counterpart in the neighbouring tetrachord above or below, at the interval of a fifth in the structure outlined here, but at that of a fourth in certain others. Thus concords have a significant role here too. If, as Ptolemy tells us later (in I.16 and II.16), instruments were in practice so tuned as to include, in a single attunement, tetrachords that differed from one another in the sizes or arrangements of their constituent intervals, such attunements were regarded by the theorists as 'mixtures' of more than one system. Hence any theoretically pure system, framed by fixed notes in the relations we have sketched, could be defined and distinguished from others simply by reference to the sizes and ordering of the intervals in any one of its tetrachords. The latter part of Book I is therefore devoted to the analysis of those forms of tetrachord that could be admitted in well constructed systems of attunement, those that defined the different kinds of system available, the 'genera' of attunement and the permissible variants of each.

But the investigation must begin, as I have said, from a study of the concords. We shall see that Ptolemy in fact regards the whole process of analysis as one of 'division', starting from the most fundamental relation, the octave, dividing it into its constituent concords, the fifth and the fourth, and then taking the smaller of them and dividing it in as many ways as are consistent with the principles, or *hupotheseis*, which he has by then outlined. The melodic legitimacy of the smaller intervals rests on their status as mathematically appropriate subdivisions of the concord of a fourth, while its status rests, in turn, on its having been derived through appropriate mathematical steps from the basic relation of the octave. Ptolemy insists that this is the only correct procedure; we should not work the other way round, constructing the fourth from the addition of independently established smaller intervals.

From the point of view of perception, as Ptolemy states (11.1–3) and as all parties to these enquiries agree, the fourth, fifth and octave are indubitably concordant, and this property belongs to no other interval within the octave. The first question, then, must be what exactly the property is that these intervals share, and which distinguishes them from the others. Greek writers broadly agree on the nature of this property, as it presents itself to the ear, though their formulations differ in detail. Ptolemy's is very brief. 'People give the name "concordant" . . . to those [intervals] which make a homogeneous (*homoian*) impression on the hearing, "discordant" to those that do not' (10.25–8). More elaborate accounts given elsewhere help to clarify the idea; here, for instance, is that of Nicomachus. '[Intervals] are concordant when the notes which bound

them are different in magnitude [i.e., in this usage, in pitch], but when struck or sounded simultaneously, mingle with one another in such a way that the sound they produce is single in form, and becomes as it were one sound. They are discordant when the sound from the two of them is heard as divided and unblended.'[2]

For the ear, then, it is the blending together of the two notes to form a perceived unity that constitutes their concordance; and the phenomenon was typically reckoned so distinct as to be unmistakable. (Aristoxenus, for instance, says at *El. harm.* 55.8–10 that the concords are much more accurately and reliably identified by the ear than are any of the discords; and the recognition of fourths, fifths and octaves is fundamental even to modern methods of tuning an instrument.) But in the context of mathematical harmonics, this characterisation of concords is not enough. The focus must be on their formal, quantitative representations, as ratios. What is required is a way of distinguishing their ratios, as such, from all others, an analysis of the purely mathematical features that only these ratios share, or the mathematical principles to which only they conform. It is in this connection, in I.5, that Ptolemy first subjects the work of his predecessors to detailed scrutiny.[3]

I.5 is mainly devoted to the exposition of some 'Pythagorean' arguments. It begins, however, with an indication of one of the difficulties into which their *hupotheseis* had led them. After identifying the three primary concords accepted as such by perception, Ptolemy notes that it also accepts three others (within the compass that will eventually concern the student of harmonics), the octave and a fourth, the octave and a fifth, and the double octave. 'The theory of the Pythagoreans,' he continues, 'rules out one of these, the octave and a fourth, by following its own special *hupotheseis*' (11.3–7). The point will be elaborated, as a criticism, in I.6. Here its consequences are not pursued; but it alerts us to the general question of what policy a theorist should adopt, if his 'rational *hupotheseis*' lead to conclusions inconsistent with the evidence of perception.

Ptolemy proceeds to set out two Pythagorean arguments, each of them designed in the first instance to show how the ratios of the three primary concords can be derived from first principles or *hupotheseis* (a few subordinate propositions are also extracted). The ratios were already well known empirically, of course; the project of these Pythagoreans, like that of Ptolemy, is to show that their possession of these values, 2:1, 3:2, 4:3, is

[2] Nicomachus *Ench.* 262.1ff. Cf. e.g. Plato *Tim.* 80b, [Eucl.] *Sect. can.* 149.18–20, Aelianus ap. Porph. *Comm.* 63.15ff., Cleonides *Eisagoge* 187.19ff.

[3] The more generalised criticisms he makes of both Aristoxenian and Pythagorean approaches in I.2 (5.34–6.14) will be considered in the context of his later and more detailed remarks.

not a casual fact, but follows from principles to which mathematical reason will assent. The two Pythagorean arguments are quite different in approach and in the concepts and assumptions on which they draw, and they must certainly come from different sources. I shall label them as arguments A and B.

Argument A (11.8–12.7) takes as its starting point ('the *archē* of their method', as Ptolemy puts it) the simple proposition that equal numbers go with notes of equal pitch, and unequal numbers with notes of unequal pitch. From this the Pythagoreans argue (*epagousin*, 11.10) that 'just as there are two primary classes of unequal-pitched notes, that of the concords and that of the discords, and that of the concords is finer (*kallion*), so there are also two primary distinct classes of ratio between unequal numbers, one being that of what are called "epimeric" or "number to number" ratios, the other being that of the epimorics and multiples, and of these the latter is better (*ameinon*) than the former' (11.10–15).[4]

Evidently the 'argument' from the *archē* to this complex proposition is not deductive. Ptolemy's word *epagousin* hints at the procedure of *epagōgē*, the abstraction or inductive derivation of general conclusions from particular instances. This conception scarcely fits the bill here, however. Probably the line of thought is roughly this. Since relations between pitches are manifestations of relations between numbers, it is to be expected that perceptibly distinct classes of pitch-relation correspond to classes of ratio that are distinct in a way intelligible to mathematical reason, and that the properties distinguishing one class of pitch-relations from another will be mirrored in properties of the corresponding ratios. More specifically, it can be anticipated that the properties which make intervals in one class 'finer' or 'more beautiful' than those in others will be matched with features, in the formally corresponding class of ratios, which make such ratios 'better' than others in some properly mathematical sense.

[4] A multiple (*pollaplasios*) ratio is one in which the greater term is a multiple of the smaller, as in 2:1, 3:1 and so on. An epimoric (*epimorios*) or 'superparticular' ratio is most simply and impressionistically described as one such that when taken in its lowest terms, the difference between them is 1. It has the form $(n+1):n$, and examples include 3:2, 4:3, 5:4. But the feature that strictly defines such a ratio, on Ptolemy's view and that ascribed here to the Pythagoreans, is that the difference between the terms is a 'simple part', an integral factor, of each. Thus whether the ratio is expressed in its lowest terms or not, as 3:2 or 6:4 or 9:6, for instance, it still fits the definition; and we are able to locate the ratio 2:1 unambiguously in the class of multiples, since though the difference between the terms is 1, it does not fall within the strict definition's scope. (The difference is not a simple part of the smaller term, but is equal to the whole of it.) An epimeric (*epimerēs*), 'number to number' or 'superpartient' ratio, for present purposes, is one that is neither multiple nor epimoric.

The more general of these propositions is fundamental to the whole project of mathematical harmonics. As to the second, more specific and perhaps more curious thesis, it evidently presupposes that there is a legitimate place in mathematics for concepts and propositions of an evaluative kind. We shall see later that Ptolemy is in sympathy with this view, but unlike the more general thesis, it is not implicated in the reasonings of all mathematical theorists alike. The dependence of argument A on evaluative considerations is indeed the feature that distinguishes it most radically from argument B.

Ptolemy next attributes to the exponents of argument A an analysis of what it is that makes epimoric and multiple ratios 'better' than epimerics. It is 'because of the simplicity of the comparison, since in this class the difference, in the case of epimorics, is a simple part, while in the multiples the smaller is a simple part of the greater' (11.15–17). Ptolemy later draws out the significance of these rather compressed remarks (initially in I.7). The task of comparing the sizes of the terms in a multiple ratio is 'simple', because the greater is an exact number of times the size of the smaller, and the smaller therefore provides a 'measure', a unit of measurement, in terms of which the size of the greater can be expressed. In the case of an epimoric ratio, this measure is constituted by the difference between the terms, which is a 'simple part' (an integral factor) of each. No element in an epimeric ratio, by contrast, is such that each term is so many times its size; and the business of comparing them is bound to be more complex and less direct.

We shall explore Ptolemy's own development of these ideas in due course, along with questions about the way in which the perceived 'fineness' of the concords is related to this specific form of 'excellence' in ratios. It is important to notice that he nowhere hints that these elaborations have a Pythagorean origin; and the indefatigable source-hunter Porphyry finds none. They are almost certainly his own. Whatever may be true of Ptolemy's version, it seems clear that if the Pythagorean explanation of these ideas went no further than the present passage reveals, the analysis of a mathematical excellence in terms of simplicity of comparison, with which Ptolemy credits them, cannot be construed as *eliminating* the role of evaluative concepts in the argument. It identifies the features that make these ratios 'better'; but the only link between their character and that of the perceptible concords is still that each is of higher status than are other forms of relation. If we eliminate the evaluative description of the ratios, preserving only the more positivistic analysis associated with it, the argument will no longer contain anything that could even suggest a connection between these ratios and the concords, let alone explain it.

The argument now proceeds by focussing on a special case of the correlation of these excellences. The octave must be in duple ratio (2:1), 'since the octave is the finest (*kallistē*) of the concords, and the duple is the best (*aristos*) of the ratios, the former because it is nearest to the equal-pitched, the latter because it alone makes an excess equal to what is exceeded' (11.21–4). Ptolemy commends this argument. He describes it as *logikōteron* (11.20–21), part of whose meaning seems to be 'distinctly rational'; it may also be intended to convey the idea that it is grounded primarily in 'calculative' or 'arithmetic' reasoning, making a contrast with the description he assigns to argument B, *grammikōteron*, 'more geometrical' (see below). Once again his own later elaborations of the argument make clear what he takes it to mean. The octave is nearest to equality of pitch, not, obviously, because the pitches of its two notes are very close together, which they are not, but because they sound almost the same and have the same melodic function, the upper note being for musical purposes only a repetition of the lower (especially 13.3–7, 15.10–12). In duple ratio 'the excess is equal to what is exceeded' in the sense that the difference between the terms is equal to the smaller term (see 15.24–5). This time, then, we are offered an analysis of the excellence to be found on each side of the correspondence, in the interval as well as in the ratio. But even this does not make the evaluative descriptions redundant. It is true that there is an affinity between the characteristics of interval and of ratio which the analyses pick out, since each is expressed in terms of some sort of equality; but the analogy remains vague and elusive. The Pythagorean argument provides nothing to confirm that 'equalities' of just these sorts are manifestations of the very same attribute, except that each in its own domain constitutes the highest kind of excellence. The evaluative aspect of this style of mathematics is an essential element in its reasoning.

Once the ratio of the octave is established, the rest will follow straightforwardly. The octave is compounded from the succession of a fifth and a fourth. Since these are concords, their ratios must be epimoric or multiple; and the only two such ratios which, when compounded, yield the ratio 2:1 are the two greatest epimorics, 3:2 and 4:3 (11.24–8). Simple arithmetic will then identify the ratios of the tone (9:8), the octave plus fifth (3:1) and the double octave (4:1); and the perceptual impression that the latter two of these are concords will be rationally acceptable, since their ratios are multiple. But as Ptolemy again points out, this cannot be said of the octave plus fourth, on the basis of these *hupotheseis*, since its ratio must be $(2{:}1 \times 4{:}3) = 8{:}3$, and is neither multiple nor epimoric (11.29–12.7).

I have surveyed this argument in a little detail, partly because some moves crucial to Ptolemy's own procedures arise from modifications and

developments of it, and partly because the intriguing form of mathematical reasoning it employs stimulates an interest in its origins. I shall say more about this matter below. Meanwhile we must look briefly at argument B, set out by Ptolemy at 12.8–24. (It appears to continue to 12.27, but the last few lines will need separate consideration.)

Ptolemy describes this second argument as *grammikōteron* (12.8), as I have said, 'more graphic' in the sense 'more grounded in geometrical representations or diagrams'. Diagrams do indeed appear in the manuscripts, but they are scarcely necessary (and their authenticity as elements of the original text is open to doubt). The argument rests on the premise, for which argument A attempted a justification, that all concordant ratios are either epimoric or multiple. In argument B this premise is assumed without being argued or even explicitly stated. The fact is unsurprising, not because the premise has been established in argument A, from which argument B is wholly independent, but because argument B is a very close paraphrase of passages in a known source, the *Sectio canonis* attributed (insecurely) to Euclid; and the passages paraphrased do not themselves explicitly state or attempt to justify the proposition. The argument offered by the *Sectio* in its support comes much earlier in the treatise, in its introductory discussion,[5] and is subsequently taken for granted.

The *Sectio canonis* is a sequence of interconnected theorems set out in the manner of a treatise in geometry. The proofs of later theorems depend on propositions proved earlier. The first nine theorems demonstrate various propositions in the mathematics of ratios, making no use of specifically musical conceptions. These begin to be introduced at Proposition 10, on the basis of a few very simple and obvious theses grounded in musical experience – that the octave, fifth and fourth are concordant, for example, that the fifth is greater than the fourth, that the double fifth is discordant while the double octave is concordant, and so on. The principle linking concords with multiple and epimoric ratios provides a bridge between these perceptually evident facts and the theorems of Propositions 1–9. The treatise then attempts to derive the values of the concordant ratios, and a number of other musical propositions, from nothing but these facts, the bridging principle, and the nine theorems in the mathematics of ratios. Ptolemy's argument B follows the reasoning, and often the wording, of the *Sectio*'s Propositions 10–12, which themselves depend principally on Propositions 2–6.[6]

Because Ptolemy's report sticks so closely to its source in the Euclidean treatise, his version is radically incomplete as it stands, containing neither

[5] *Sect. can.* 149.16–20.
[6] For a fuller discussion see Barker (1981), and compare Barbera (1984 and 1991).

the *Sectio*'s argument for the principle linking concords with multiple and epimoric ratios, nor the proofs of the essential theorems about ratios contained in its Propositions 2–6. His version, like that of its source, involves an indefensible logical blunder, arguing that since the double fifth is not concordant, its ratio cannot be multiple (12.10–11, *Sectio* Proposition 11). Nothing has been said to support the premise which this move would require, that all multiple ratios are ratios of concords. Neither the writer of the *Sectio* nor any other theorist in the tradition would have had any reason for supposing that they are, and in fact some are not. (The ratio 5:1, for example, is the ratio of two octaves and a major third, which by Greek standards is a discord, and 7:1 is that of three octaves less a rather large tone.) Ptolemy has a sharp eye for errors of reasoning and is seldom reticent about exposing them. He had no reason to condone or overlook the one involved here – at 13.23–14.1 he explicitly alludes, for a different purpose, to the fact which undermines the present argument, that is, that not all multiple ratios are concordant.

The fact that he lets the move pass here without comment is a tolerably clear indication, I think, that the argument as a whole was of little concern to him. Argument A, on the other hand, is close to the centre of Ptolemy's focus. He will develop its ideas later, as I have said; and even though he will not accept its premisses or its reasoning precisely as they stand he goes out of his way to express approval of aspects of its strategy. Its initial principle is 'most appropriate' (*oikeiotatēn*, 11.8) and its later procedure 'distinctly rational' (*logikōteron*, 11.20–21). In Ptolemy such compliments are rare.

The reasoning used by the *Sectio canonis* to establish the ratios of the concords differs in several ways from that of argument A. The most significant distinction is that neither in its (quite unconvincing) argument for the principle linking concords with restricted classes of ratio, nor indeed anywhere in the theorems that follow, does it call on conceptions of an evaluative kind. Its project fails, since its derivation of the values of the concords cannot proceed without the conclusion which the flawed reasoning of Proposition 11 is designed to establish. In fact the task it sets itself is impossible; there can be no such derivation on the basis of hard-headed mathematics of this sort alone. The reasoning of argument A, by contrast, once its general strategy and its evaluative suppositions are accepted, has the merit of being sound.

Argument B, which I shall not now discuss further, is followed by a brief and intriguing appendix. Let us call it argument C. Ptolemy presents it (12.24–7) as continuous with argument B itself; they are separated only by a comma in Düring's edition. Argument C runs as follows. 'Since the tone is accordingly shown to be in epogdoic ratio [i.e., in the ratio 9:8],

they reveal that the half-tone is unmelodic (*ekmeles*), because no epimoric ratio divides another proportionally as a mean, and melodic intervals (*ta emmelē*) must be in epimoric ratios.'

Now this does indeed have a connection with argument B and the *Sectio*. A roughly analogous thesis is proved in the *Sectio* at Proposition 16; and argument B, with its source, has already made use of the theorem underpinning it (Proposition 3 in the *Sectio*). But what that theorem shows is that there is no mean proportional of any description between terms in an epimoric ratio; that is, where X:Z is epimoric, there is no ratio of integers X:Y such that X:Y = Y:Z. It does not restrict itself, as argument C does, to the claim that X:Y and Y:Z cannot themselves be epimoric. This will of course follow *a fortiori*, but if argument C were intended as a paraphrase of the *Sectio*'s Proposition 16, we would have to conclude that Ptolemy had confused the issue in an uncharacteristic way.

In fact, however, argument C cannot belong with the *Sectio* and argument B at all. The *Sectio* says nothing at all about the characteristics of melodic intervals in general. It does not even use the term 'melodic' (*emmelēs*) or any equivalent expression, or raise the issues surrounding the concept in any way whatever. In Ptolemy's usage, 'melodic' intervals are those intervals smaller than the perfect fourth, which can properly appear in an attunement as elementary scalar steps. It is not just that the *Sectio* offers no rules about such intervals. Our two texts diverge more radically, since in fact the rule asserted in argument C is flatly inconsistent with the *Sectio*'s conclusions and the procedures by which it reaches them. Both the enharmonic pattern of attunement presupposed in Propositions 17–18, and the diatonic system analysed in Proposition 20, contain scalar steps that are not epimoric.[7]

Argument C, then, cannot come from the same stable as argument B. It is much more likely, I think, to belong with argument A, whose evaluative preconceptions would give a reason for restricting the category 'melodic' to intervals with epimoric ratios, and for keeping the 'inferior' class of ratios, the epimerics, as a dustbin for unmelodic relations. Now Ptolemy does not identify the source of argument A. But if argument C is to be detached from argument B, as I think it must, there is a good case to be made for the claim that it goes back to the fourth century BC, and that its source is Archytas. It will turn out that if this is correct, the links between

[7] Prop. 20 constructs diatonic tetrachords whose intervals have the ratios 9:8, 9:8, 256:243, and the last of them, the so-called *leimma*, is not epimoric. The enharmonic division presupposed in Props. 17–18 has an upper interval amounting to a true ditone (9:8 × 9:8 = 81:64), which is not epimoric. Its two lower intervals are divisions of the remaining *leimma* of 256:243; and there is no way of dividing this ratio into two epimorics.

arguments A and C are strengthened, and there will be powerful reasons for thinking both of them Archytan in origin.

I cannot pursue the necessary investigations in much detail here; but the matter is of sufficient interest, both in itself and for what it tells us about Ptolemy, to call for a brief summary of them.[8] First, if argument C is independent of argument B, it requires its own independent support for the proposition about mean proportionals in epimoric ratios. This the Archytan hypothesis provides, since we know from Boethius (*Inst. Mus.* III.11) that Archytas gave a proof of the relevant theorem. Boethius' version differs somewhat from that of the *Sectio*,[9] suggesting that the proofs may have been transmitted in two distinct traditions, one through the *Sectio*, the other, perhaps, through Ptolemy's informant on Archytas (who was probably Didymus the Younger, a musical writer of the first century AD).

Secondly, the first person to subscribe to the rule confining melodic intervals to the class of epimorics must have had plausible theoretical reasons for doing so, since on the face of it it is highly contentious. It is inconsistent with the systems propounded by Philolaus, Plato, the *Sectio canonis* and many others, and is likely to have been hard to reconcile, also, with the procedures of practical musicians.[10] The earliest theorist we know of who apparently adopted a version of this rule is Archytas himself, and Ptolemy indicates that he did indeed offer argumentative support for it. He accepted it because he believed 'that the commensurateness of the excesses is a characteristic of the nature of melodic intervals' (30.12–13). The sense of this dark utterance is unpacked by Ptolemy on his own account elsewhere (particularly 16.12–21). It means that the difference between the terms (the 'excess' of the greater over the smaller) must be a 'measure', a 'simple part' or integral factor, of each of the terms themselves; and that condition defines an epimoric ratio.

It must certainly be on some such consideration as this that argument C depends. Both the main principles underlying it, then, and the notion of the 'melodic' itself, have Archytan credentials, or at least what Ptolemy took to be such; and Archytas is almost certainly the originator of the rule

[8] The issues sketched here are more fully explored in Barker (1994).

[9] See the discussion in Knorr (1975) ch. 7.

[10] For Aristoxenus' comments on tuning-procedures, and on the ear's capacity to recognise concords more easily and accurately than discords, see *El. harm.* 55.3–56.12; cf. [Eucl.] *Sect. can.* prop. 17. If methods of this kind are used to construct tones and 'semitones', and are applied accurately, every 'semitone' will turn out to be either a *leimma*, in the ratio 256:243, or an *apotomē* (the difference between a *leimma* and a whole tone), in the ratio 2187:2048. The suspicion that Aristoxenus' 'method of concordance' reflects procedures used regularly by musicians themselves is confirmed – at least for the later period – by Ptolemy's own remarks at 39.14–19.

it asserts. But the ideas used by Archytas to support that rule take us back to argument A, since it is precisely the feature of epimoric ratios to which they draw attention that was used in argument A to indicate the special 'simplicity' of such ratios, and to confirm their superiority to epimerics. Thus if argument C is Archytan, so probably is argument A.

There are two rather more general pointers to this conclusion. First, these arguments are attributed to 'Pythagoreans'. Ptolemy later describes Archytas as a Pythagorean (30.9–10), but uses the term of no other named individual. In particular, he does not attach the label to either of the other theorists, Didymus and Eratosthenes, who appear from what he tells us to have adopted (not quite consistently, in Eratosthenes' case) a similar rule about melodic intervals. It would indeed have scarcely been appropriate to either of them, even given the elastic usage of the title in the hands of writers on harmonics. Secondly, I noted above that Ptolemy is uncharacteristically flattering about features of argument A, even though he will reject some aspects of it. The only comparable accolade he allows himself elsewhere in the *Harmonics* is presented to Archytas. 'Archytas of Taras, of all the Pythagoreans the most dedicated to the study of music, tried to preserve what follows the principles of reason not only in the concords but also in the divisions of the tetrachords . . .' (30.9–12). The reverberations of this fanfare are admittedly damped down almost at once, as Ptolemy proceeds to criticism; but even there he allows that Archytas 'is in most cases well in control of this sort of thing' (30.14–15), only occasionally lapsing into error. Further, the remark at 30.9–12 implies that Archytas 'tried to preserve the principles of reason' in connection with the concords, among other things. This can only mean that Ptolemy associated him with the postulation of certain 'rational *hupotheseis*' concerning the concords; and unless Ptolemy is deliberately suppressing some wholly different suggestions he believed Archytas had made, they can be none other than those set out in the present chapter, 1.5. It is unlikely that they are those of argument B, since Porphyry, who appears to have used the same source as Ptolemy here, unambiguously attributes it to Euclid. That leaves the *hupotheseis* of argument A as the only plausible candidates. I submit, then, that there are good (not of course conclusive) reasons for linking argument A with argument C, and for giving Archytas the credit for devising them both.

In 1.6 Ptolemy presents three criticisms of the Pythagorean approach. Only two of them are directed against the arguments set out in 1.5; they expose simple but serious flaws. In the final section of the chapter he turns his fire on a piece of Pythagorean reasoning that has not previously been mentioned.

The first criticism (13.1–23) is by now familiar. The basic Pythagorean *hupothesis* about the concords, that all their ratios must be multiple or epimoric, is at odds with the plain evidence of perception in the case of the octave plus fourth, which is undoubtedly heard as a concord, but whose ratio is 8:3. (It is notable that in the *Sectio Canonis*, for example, this embarrassing interval is not even mentioned.) Ptolemy's repeated allusions to this difficulty reflect the significance he attaches to it; this is the third time it has been mentioned in as many pages of text. Here he is not content merely to assert that the ear accepts the interval as a concord; he offers an argument. Its concordance can be guaranteed by reflection on an important property of the octave. 'For it is always true of the concord of the octave, whose constituent notes do not differ in function [*dunamis*, alternatively 'power' or 'character'] from a single note, that when it is attached to one of the others it keeps the form of the latter unaltered . . . And if one takes a note that lies in the same direction from both extremes of the octave, downwards from both of them, or again upwards, as it is to the nearer of them so it appears to be to the further' (13.3–10). That is, if a note lies, for example, a fourth below the lower boundary of an octave, it will also be heard, when played together with the note at the octave's upper boundary, as if it stood in the same relation to that note too. The addition of an octave to the interval makes no difference to its character.

Similar claims about the octave are made by other writers;[11] but we are entitled to wonder whether the argument, for all its plausibility, really adds anything substantial to the point from which Ptolemy began, that the octave plus fourth is perceived as a concord. The argument, it may be objected, is wholly inductive. It appeals only to our experience, which uncovers no exceptions to the rule that intervals retain the same perceptible character when supplemented by an octave as they do when taken alone. This generalisation is merely false, unless the octave plus fourth is itself perceived as having the same character as the simple fourth; and that, it might seem, is precisely what is at issue. A determined Pythagorean might contend that the induction fails, on the grounds that this is not how the octave plus fourth strikes his ear.

In this form, however, the objection cuts little ice. For one thing, so far as we know, such claims about the perceived character of this interval were never made. If they had been, they would probably have been dismissed as evidence of mere peculiarities in the auditory apparatus of the claimant. The fact that to normal human hearers the interval *sounds* like a concord was never in dispute. Hence Ptolemy's induction, if it is one, can be allowed to stand.

[11] For instance at Aristox. *El. harm.* 20.17–21.

But the Pythagorean has what appears to be a stronger card to play. He can insist that the inductive move, though sound as far as it goes, is strictly irrelevant. The issue is not whether this interval is standardly heard as a concord, but whether it really is one; and here the decisive criterion must be the formal or 'rational' one, not the character of impressions received by the senses. Perception is not authoritative on the question whether this or any other interval really is a concord, that is, whether its ratio is correctly understood as belonging to a privileged, 'better' class of ratios. If the ratio of the octave plus fourth, despite its oddity, really should be construed as belonging to this class, then the Pythagorean conception of the principles governing the form of such ratios must indeed be mistaken. But Ptolemy's inductive procedure rests on appeal to perception alone, and does nothing to establish the point he requires.

Ptolemy has the choice of two kinds of rejoinder to this criticism. The first would involve denying that the move under attack is an inductive one. It might be conceived, instead, as enunciating a definition of the role or 'function' of the relation of the octave in anything that is recognisable as a musical system; and it would claim that no sense can be made of musical procedures if the octave is not conceived in this manner. This kind of response could, I think, be defended; but there is no sign of it in Ptolemy's text, and an exploration of it here (which would necessarily be lengthy) would be out of place.

The second rejoinder hangs directly on Ptolemy's general view about the relation between the 'criteria' of reason and perception, and their roles in harmonic science, which were outlined in I.1. The data of perception are rough and ready, but reason has no right to dismiss them as wholly false. Its task is to 'bring them to accuracy', on the assumption that counterparts of the kinds of relation broadly gestured at by perceptual impressions are indeed there in mathematical structures to which the perceived relations approximately or exactly correspond. Without this assumption reason is powerless, since there is nothing in mathematical reasoning alone to show that certain privileged ratios correspond to perfectly formed musical intervals, or indeed that there are such things as musical intervals at all. Concepts such as 'musical', 'concordant' and so on can enter the mathematical repertoire only as the result of its cross-fertilisation with the realm of aesthetic perception, and only in so far as mathematical formulae are construed as interpretations or 'rationalisations' of relations first classified by the musical ear.

In Ptolemy's view, then, the Pythagoreans' embarrassment over the status of the interval in question points to general issues of fundamental importance. The scientist's task, as he understands it, is 'to show that the works of nature are crafted with reason . . .' (5.19–21). The 'works of

nature' are those presented to perception; and the development of conceptions of rational order which are different from the order manifested in perceptible things would at best be irrelevant to the scientist's enterprise. What must be shown is that perceptible beauty is a reflection of rationally intelligible form, and the scientist's *hupotheseis* must be ones whose consequences are not at odds with the phenomena they are designed to explain (see particularly 5.13–17). The problem of the octave plus fourth provides Ptolemy with the main grounds for his rather sweeping comment in I.2, that the Pythagoreans 'did not follow the impressions of hearing even in those things where it is necessary for everyone to do so' (6.1–2).

Two writers quoted by Porphyry in connection with I.2 help to clarify the point. Even the Pythagoreans cannot set out on their enterprise without relying on perception for certain elementary facts, such as the fact that the octave, fifth and fourth are concords. By what right, then, do they reject perceptual evidence of just the same sort in other cases? For one of Porphyry's (and probably Ptolemy's) sources, Ptolemaïs of Cyrene, this manoeuvre condemns them outright. 'These people are wholly refuted by their practice of accepting something perceptible at the beginning, and then forgetting that they have done so' (Porph. *Comm.* 25.13–14). Her fuller characterisation of them at 25.25–26.1 is picked up in almost the same words by another source, Didymus; but he contrives to represent their strategy in a rather more favourable light. 'They adopt [from perception] certain kindling sparks . . . and construct the theorems that are put together out of them through reason on its own, taking no further notice of perception. Hence on occasions when only what follows rationally is carefully preserved, and perception bears witness against it, it is possible for them to be not in the least disturbed by this sort of discord, but to pin their faith upon reason and dismiss perception as going astray' (Porph. *Comm.* 26.18–24). This account of their approach has a detectably Platonist ring. Perception is useful, but only in so far as it can give us hints of the existence and nature of an independent, rational order, of which the perceived phenomena may be only a distant and distorted echo. Once our mind has developed the resources needed to explore this order alone, it has no further need of perception, and perceptual evidence cannot be used to refute its conclusions.[12] This position is arguably coherent. What it is not, from Ptolemy's point of view, is relevant to the task in hand. It is the rational order *in* the phenomena that the scientist is seeking to uncover, not some other.

Ptolemy's second criticism (13.23–14.1) is simple and apparently devastating. Even if we allow the principle that all concords must have multiple or epimoric ratios, what is there to distinguish those multiple or

[12] Compare e.g. Plato *Phaedo* 65e6ff., 73c1ff., *Rep.* 510d5ff.

epimoric ratios that correspond to concords from those that do not? (Ptolemy offers the ratios 5:1 and 5:4 as examples of the latter sort.) The two groups have just the same general mathematical characteristics, the same 'form', as Ptolemy puts it; there are no mathematical grounds for drawing a significant line between them.

The problem is a serious one, since there is no such mathematical distinction to be found. In the page that follows (14.1–15.2) Ptolemy examines a set of Pythagorean manoeuvres that appear to have been designed to tackle the difficulty from another angle. It is one of the most savage passages of critical writing in the *Harmonics* – Ptolemy positively devours the unfortunate theorists, spicing the funeral meats with peppery sarcasm. Crude though their procedures certainly are (even as reported in Porphyry's less polemical version), Ptolemy's unmerciful assault should not be allowed to mislead us. In one important respect their approach is closely parallel to his own.

Their strategy is to look for a way of grading the ratios of intervals for their greater or lesser degrees of concordance. Ptolemy summarises the gist of their procedure as follows. 'From each of the first numbers that make up their ratios they subtract a unit, on behalf of the similarity arising from both, and the remaining numbers they posit as belonging to the dissimilarities; and the smaller these turn out to be, the more concordant they say they are' (14.2–6). *Kai panu geloiōs*, he goes on, 'and this is utterly ludicrous'; and he proceeds to tear them apart. He offers no further direct exposition, and perhaps none is required. Porphyry, however, presents the Pythagorean argument in some detail (*Comm.* 107.15–108.21), and identifies his source, which is undoubtedly Ptolemy's too. 'Some of the Pythagoreans, as Archytas and Didymus relate,' he begins (*Comm.* 107.15); which can hardly mean anything except that he found the report in a work by Didymus, where it was represented as an account, given by Archytas, of procedures adopted by his predecessors or contemporaries.

Porphyry's account has interesting features, some of which tend to support the view that the report has genuinely fourth-century origins. I cannot pursue those issues here. The procedure is straightforward, and as Ptolemy says, it looks mathematically absurd. We take in its lowest terms the ratio of some concord, for instance that of the fifth, 3:2. From each of the terms we subtract a unit, these being designated the 'similars' (*homoia*, Porph. *Comm.* 108.7), while the remainders are summed to constitute the 'dissimilars' (*anhomoia*, *Comm.* 108.8). The 'similars', taken together, always add up to 2; in the case of the fifth, the 'dissimilars' amount to 3. The smaller the dissimilarity, the greater the degree of concordance. Thus in the octave, where it is 1, the degree of concordance is greatest of all, whereas the concordance of the fourth, where the dissimilarity is 5, is less than that of the fifth. Evidently the allocation to each

such interval of an element of similarity and an element of dissimilarity reflects the intuition that notes jointly forming a concord must have something in common, and yet are different notes. Moreover, while no serious mathematical sense can be made of the procedure, the results listed above are plausible enough; and it would work rather well for epimoric ratios in general, whether they are those of (Greek) concords or not. (Thus the dissimilarity in an interval of ratio 5:4, a major third, is 7, making it less concordant than the perfect fourth, whose dissimilarity is 5, but much more concordant than the tone, whose ratio is 9:8 and whose dissimilarity is therefore 15.) The greater the terms, the less concordant the interval; and Ptolemy himself will subscribe to a closely related thesis (16.17–21), though his reasons are of course entirely different.

We need not spend long on Ptolemy's criticisms. He points out first (14.6–16) that it should make no difference whether the ratios are taken in their lowest terms or not; they remain the same ratios and correspond to the same intervals. Then if, for example, we assign the number 6 to the lower term of each ratio – and why should we not? – we shall find that the results of the Pythagorean procedure come out differently. If, for instance, we call the ratio of the octave 12:6 instead of 2:1, and that of the fifth 9:6 instead of 3:2, and subtract 6 (or any other number) from each term of each ratio to represent the 'similarities', the remaining 'dissimilarity' will be greater in the octave than in the fifth. Ptolemy is obviously insinuating that the Pythagoreans' insistence on taking each ratio in its lowest terms is arbitrary. I have suggested elsewhere that it is not;[13] but it seems clear, at least, that no reasons were offered for it in Ptolemy's source. None appears in Porphyry's fuller version.

Secondly, Ptolemy argues (14.16–15.2) that some of the conclusions generated by the procedure are simply wrong. In particular, the octave plus fifth (3:1) will turn out to be more concordant than any of the others apart from the octave, more so even than the simple fifth (3:2) and the double octave (4:1). Yet the fifth, since it is simpler, is surely a purer concord than the octave plus fifth; and the double octave stands to the latter in the same mathematical relation as does the octave to the former, so that it must exceed the octave plus fifth in its degree of concordance to the same extent as the octave exceeds the fifth.

I shall not comment further on the validity of Ptolemy's criticisms. The point I want to emphasise is of a different sort, and is brought out by the contentions he makes in the last phase of his discussion. The Pythagoreans' computations, in his judgement, are merely puerile. But his comments in 14.16–15.2 show that he shares the assumption from which they begin, that some of these concordant intervals are 'more con-

[13] In *GMW2*, 35 n.29.

cordant' than others; and he will develop the idea in I.7, in his attempts to find a way round the problem of distinguishing concordant ratios mathematically from ratios of discords. This is not a notion of which any use is made by other theorists known to us from the period before Ptolemy. But a claim of a similar sort was implicitly attributed to the Pythagoreans earlier, in argument A of I.5, where substantial results were derived from the thesis that the octave is the 'finest' of the concords. I gave reasons for the view that argument A comes from Archytas, probably through the medium of Didymus. So does the present account, as we have seen, though the procedures it records are apparently not those of Archytas himself. If the attribution in Porphyry is correct, he at least took the trouble to report it. We know nothing of his views about its cogency.

It begins to seem likely, then, that Didymus' discussions of Archytas' writings were quite extensive,[14] and that Ptolemy studied them closely. He found in them much food for thought, not merely faults to criticise. Three very distinctive features of his subsequent argumentation seem to have origins in this source: his way of connecting concords with multiple and epimoric ratios, his insistence that melodic intervals must also have epimoric ratios, and his policy of grading intervals on a scale analogous to the Pythagoreans' gradation of concordance. The second of these features seems to be reflected in the work of two theorists of the intervening period, one of whom is Didymus himself. Of the first feature and the third there is no trace elsewhere. Didymus, we may guess, had access to a group of documents which he at least believed were derived from Archytas, but which had been lost from sight in the Hellenistic period.[15] Part of Ptolemy's originality may lie in his recognition of the possibilities that had lain dormant for so long, in ideas to which Didymus' account gave access. Perhaps they were imperfectly conceived and crudely stated, but through Ptolemy's transformations of them they became key pivots of his procedure. We shall later find reasons for considering seriously the suggestion that certain much more general characteristics of his approach are also drawn from this source.[16]

[14] It is possible that he is the same Didymus who is mentioned by Clement of Alexandria (*Strom.* I.16) as the author of a work *On Pythagorean Philosophy*.

[15] For further discussion of his origins and the nature of his work, see Barker (1994a), especially 64–73.

[16] This is not to say that I accept the view of Porphyry, who comes close to accusing Ptolemy of plagiarism. Most, indeed virtually all of the content of Ptolemy's treatise is taken, he says, from older sources; and he goes on: 'At any rate, at many points he has transcribed the work of Didymus, *On the difference between Pythagorean and Aristoxenian musicology*, without ever mentioning the fact.' It is true that he hastens to add: 'One should not criticise him for this, since everyone uses things that are well said, as being common property', but it hardly erases the impression that he considers what Ptolemy wrote to be largely second-hand. See *Comm.* 5.7–16. It is worth pointing out that the extensive quotations Porphyry offers from earlier writers do little or nothing to substantiate that claim.

5 The ratios of the concords: (2) Ptolemy's *hupotheseis*

'It would not be right to attribute these errors to the power of reason, but to those who ground reason in faulty *hupotheseis*' (*tois mē deontōs auton hupotithemenois*, 15.3–4). This statement stands as a preface to Ptolemy's own account of the concords in I.7. The Pythagoreans' errors have been shown up principally by recourse to the evidence of perception. That strategy implies that the test of perception is to be trusted; but the errors it reveals should not deter us from the quest for rational principles. Hence where propositions derived from supposedly rational *hupotheseis* are at odds with the perceptual data, neither reason as such nor the senses should be blamed for the conflict, nor should either be dismissed as unreliable in its own sphere of competence. The proper conclusion is that the *hupotheseis* have been misconceived or wrongly applied.

Ptolemy's exposition of the correct principles begins with a three-fold classification of musical intervals. 'Preeminent in excellence is the class of homophones, second that of concords, and third that of the melodic. For the octave and the double octave plainly differ from the other concords as do the latter from the melodic, so that it would be more appropriate for them to be called "homophones". Let us define homophones as those which, when played together, create for the ear the impression of a single note, as do octaves and those composed of octaves; as concordant those closest to the homophones, like fifths and fourths and those composed of these and the homophones; and as melodic those closest to the concords, like tones and others of that sort. Thus in a way the homophones go together with the concords, and the concords with the melodic' (15.6–17).

These 'definitions' are admittedly vague. In view of their objective, which is to classify intervals as they present themselves to the hearing, a certain vagueness is inescapable, since it is characteristic of the senses that they 'discover what is approximate and adopt from elsewhere what is accurate', and that to achieve accuracy perception 'needs, as it were as a crutch, the additional teaching of reason' (3.6–7, 19–20). Ptolemy's distinction between octaves and other concords is not wholly new, though his use of

the term *homophōnos* in this connection has no known precedents.[1] The two features of his account which need particular emphasis are ones we have already encountered. First, the classification is presented as one that distinguishes classes of interval by their degrees of 'excellence' (*aretē*, 15.7), in a way that echoes the evaluative approach of argument A in I.5 (see pp. 60–63 above). Secondly, the usual Greek assumption that concords are sharply marked off from other intervals by perceptual criteria is apparently undermined by the contention, opaque though it is, that the concords 'go together with' the melodic intervals, just as the homophones go with the concords. There seems to be a suggestion here that there is a continuous gradation between these classes, rather than a series of abrupt steps. This notion will be important in the sequel; and it is reminiscent, if no more, of the Pythagoreans' attempts to grade intervals in respect of their degrees of concordance (pp. 71–3 above).

Ptolemy now says explicitly that he will adopt the same 'initial principle' (*archē*) as the Pythagoreans of argument A, assigning equal numbers to notes of equal pitch, and unequal numbers to those of unequal pitch, 'since that sort of thing is self-evident' (15.19–21, referring back to 11.8–10). From this point on the reasoning becomes quite complex, and the passage is best quoted in full.

Then since it is in accordance with this principle that we should measure and compare the differences that have been set out between unequal-pitched notes by their closeness to the equalities, it is at once clear that the duple ratio [2:1] is closest to this equality, since it has an excess equal to and the same as the number that is exceeded; and of the homophones the most unitary and finest is the octave; so that we should fit to it the duple ratio, and to the double octave, obviously, the double duple ratio, that is, the quadruple [4:1], and so on for any others that are measured by the octave and by duple ratio. Again, after the duple ratios, the nearer to equality are those that most nearly divide that one [i.e., duple ratio] in half, that is, the hemiolic [3:2] and the epitritic [4:3]. For what divides most nearly into halves approximates to dividing into two equals. After the homophones the first of the concords are those that divide the octave most nearly into halves, that is, the fifth and the fourth, so that we can again conclude [*tithesthai*] that the fifth is in hemiolic ratio and the fourth in epitritic. Second are those formed by putting each of the first concords with the first of the homophones, the octave plus a fifth in the ratio put together from the duple and the hemiolic, which is the triple [3:1], and the octave plus a fourth in the ratio put together from the duple and the epitritic, which is that of 8 to 3. For the fact that this ratio is neither epimoric nor multiple will now be no embarrassment to us, since we have adopted no preliminary *hupothesis* of that sort (*mēden ge toiouto prohupotetheimenous*). (15.22–16.12)

1 *Homophōnos* most commonly means simply 'unison'; and in the majority of authors the octave is described straightforwardly as a concord, *sumphōnia*, though it is recognised that it has special features of its own. (For some of them see e.g. Aristox. *El. harm.* 20.17–21, [Ar.] *Problems* XIX.35, 39, 41, Thrasyllus ap. Theon Smyrn. 48–9.)

As in the Pythagoreans' argument A, the concept on which the argument pivots is that of equality. But it is developed in several new ways, and it is not clear that it plays the same role at each step of the analysis, or in its immediate sequel (16.12–21), to which we shall turn shortly.

The first step attempts to bridge the gap, which was left open in argument A, between the initial principle, the *archē*, and what follows. Notes of equal pitch correspond to equal numbers, and any two such notes are for the purposes of harmonic science the same; their identities are completely fused. Ptolemy proposes to grade pairs of notes whose pitches are not the same by the closeness of the relation in which they stand to the equality displayed by these unisons. As before, this closeness to equality is not represented by adjacence in pitch, or by the near-equality of the two terms in the corresponding ratio. The ratio closest to equality is duple ratio, 2:1, we are told, because the difference between the terms is exactly equal to the smaller term. But why should the equality of these elements, in particular, be the relevant one? The question is not explicitly addressed here, and we shall return to it, noting only one obvious point for the present. In any ratio, the greater term is the sum of the smaller term and the difference between the terms. Here, since the difference and the smaller term are equal, the sizes of the terms can be compared in the simplest possible way. We construct the greater term by taking the smaller twice over.

On its next appearance, the notion of equality seems to be differently used. The ratios 3:2 and 4:3 are described as the next nearest to equality because they most nearly 'divide in half' the ratio 2:1. (On any interpretation, this will be true only if we assume that epimeric ratios are ruled out as irrelevant.[2] This has not so far been stated, let alone proved. It is another point to which we shall return.) Here, then, the relevant equality or near-equality does not characterise the relation between the smaller term of a ratio and the difference between the terms, or indeed any relation internal to the ratio itself. It is because of their relation to another ratio, the duple, that the hemiolic and the epitritic, 3:2 and 4:3, are the next in order of excellence.

Let us consider a little more closely what Ptolemy means by 'dividing the ratio 2:1 in half'. It will be convenient to express the three ratios involved, 2:1, 3:2, 4:3, in such a form that each has the same lower term, for instance as 12:6, 9:6, 8:6. Now clearly 'dividing 12:6 in half' does not mean 'finding the mid-point between 12 and 6', since that is straightforwardly 9, and no approximation would be needed. In any case, this

[2] The epimeric ratios 338:234 and 324:234, for instance, when compounded, also give the result 2:1, and they are obviously more nearly equal than 3:2 (=351:234) and 4:3 (= 312:234).

arithmetical operation has no role in the composition and division of ratios in a harmonic context. Ptolemy must have in mind the operation that would exactly halve the musical *interval* which the ratio 2:1 represents, the octave; and that is done by finding not the arithmetical midpoint between the terms but their geometric mean, or 'mean proportional'. This will be a term, T, such that 12:T = T:6; and it is the square root of 72, which lies between 8 and 9, and does indeed need to be approximated. The ratios 9:6 and 8:6 are the closest epimoric approximations to a 'half', in this sense, of the ratio of the octave. When 2:1 is factorised as 3:2 × 4:3, the octave has been divided into the two most nearly equal parts that are acceptable, if indeed these ratios must be epimoric. (No other such division at all, in fact, will be into intervals with epimoric ratios.)

If we now return to the relation specified earlier between the ratio 2:1 and equality, the difference between that case and the present one will be seen more clearly. To make the two cases analogous we would have to reinterpret the sense in which the lower term in duple ratio is 'halfway' to equality with the greater. If we express the ratio again as 12:6, there is an obvious sense in which 6 is halfway to equality with 12; it is halfway to 12 from zero. But that, as we have seen, is not the sense in which 9 and 8 are 'approximately halfway' between 6 and 12. They are approximations to the mean proportional between these terms. Patently 6 is not even an approximation to the mean proportional between zero and 12, since that notion makes no sense; and though it is of course the mean proportional between 3 and 12, that fact has no bearing on Ptolemy's line of argument. 'Equalities' of different sorts are indeed in play.

Ptolemy turns next to the larger concords, but says nothing about them beyond what is obvious – that each is formed by putting one of the primary concords together with the first homophone, the octave. He identifies their ratios on this basis, and asserts that the peculiar form of the ratio of the octave plus fourth, 8:3, poses no problems for him (as it did for the Pythagoreans), since he has not adopted the *hupothesis* that all ratios of concords must be epimoric or multiple. That claim is true, as far as it goes; but it is curious that Ptolemy explains the status of the ratios of the larger concords no more fully, in view of the emphasis he laid earlier on the difficulties created for the Pythagoreans by the octave plus fourth.

There are two problems here. The first and more general one is that of identifying the feature shared by the ratios of the greater concords which gives them the required sort of mathematical 'excellence'. The second and more particular is to explain how the anomalous ratio of the octave plus fourth can be unproblematically accepted as that of a concord. It is hard to offer Ptolemy much help with either of these difficulties, on the

basis of what has so far been said in this passage. To construct the ratio of one of the greater concords, we begin by increasing a given term by an amount equal to itself. This gives a term in the ratio 2:1 to the first, its octave. We then increase this second term, in such a way as to reach a point approximating to the mean proportional between it and the term bounding the next octave. So much is tolerably clear; but we are surely entitled to expect some explanation of how the ratio constructed by this rather complex operation is to be conceived as sufficiently simple and mathematically excellent to count as 'concordant'.

As to the second problem, we have seen that in his analysis of the fourth and the fifth, Ptolemy is unquestionably relying – though he has not yet said so – on some version of the thesis that the ratios of concordant intervals must be epimoric, and we shall see below how this rule emerges. If adopted without qualifications, however, it is bound to cast doubt on the status of the ratio 8:3. Ptolemy confidently asserts that he has adopted no principles that would make this ratio problematic. No doubt he is at liberty to restrict the scope of the rule to 'simple' concords, and to explain the mathematical status of the greater concords, if he can, as arising from the characters of the ratios from which they are compounded. But we may reasonably feel some puzzlement at the strategy he has employed in accounting for the concordance of the fourth and the fifth themselves. It depends on their relation to the octave, and involves a certain form of approximation. Yet approximations seem out of place in this mathematical phase of the account; and from a perceptual point of view the concordance of these intervals seems not to be experienced in some kind of comparison between them and the octave, but to be given immediately in the perception of each interval by itself. Since we perceive concordance as an internal feature of each of these intervals, not one constituted by their relation to some other interval, we should expect it to be analysed mathematically in terms of some relation between their own elements.

So far as this last issue is concerned, the truth, I think, is that Ptolemy is trying to do too many things at once. The fifth and the fourth stand in a relation to the octave that is central to harmonic analysis. Each of them also strikes the ear as a particularly well unified relation in its own right. These, on the face of it, are two distinct facts. One of them may have a part to play in helping us to understand the other, but Ptolemy's explanation of the first seems ill adapted to the task of providing an adequate explanation also for the second. The immediate sequel, however, suggests a way in which some of the damage can be repaired. It takes us beyond the topic of the concords, and we shall at this stage consider its treatment of melodic intervals only in so far as it throws light on these earlier questions.

Melodic intervals, from an informal point of view, are those which can function as individual steps in a musical scale. More formally, they are those which are smaller than the smallest concord (the fourth), and which can be put together in various combinations to form tetrachords. (A tetrachord, for these purposes, is a sequence of three intervals defined by four notes, jointly spanning the concordant interval of a perfect fourth.) The first relevant claim that Ptolemy makes is that the ratio of any melodic interval must, once again, be such that the difference between its terms is an integral factor of the terms themselves. Hence it must be epimoric; only in such ratios are the differences 'simple parts' of the terms. The crucial point comes near the end of the passage. 'Of these ratios too, those that make divisions most nearly into halves must be more melodic, for the same reason as are all those whose differences contain larger simple parts of the things that are exceeded; for these, too, are nearer to the equal, just as the half is nearest of all, then the third, and then each of the others in succession' (16.17–21).

Now this talk of 'making divisions most nearly into halves', and of those that are 'nearer to the equal' is obviously reminiscent of what was said about the fourth and the fifth; and Ptolemy plainly intends it to be so. The phrases 'of these ratios too', 'for the same reason', and 'for these, too' all refer back to the preceding discussion. But Ptolemy's use of the notion of 'equality' has now shifted again. The concept of 'approximate halving' entered the analysis of the fourth and the fifth as a representation of their relation to the octave. Here, however, those that 'make divisions most nearly into halves' and are therefore 'more melodic' are not defined as those which most nearly provide the mean proportional between the terms of some other ratio (which would presumably, in this context, be that of the fourth). They are 'all those whose differences contain larger simple parts of the things that are exceeded'. This can only mean that one epimoric interval corresponds to a 'more melodic' interval than does another, if the difference between its terms is a larger factor of its smaller term. We have apparently gone back to the kind of 'equality' that was in play during the discussion of the octave. The ordering of these lesser ratios is then very straightforward. The smaller the terms, the more melodic the corresponding interval, since in the ratio 5:4, for instance, the difference is one quarter of the smaller term, while in the ratio 6:5 it is one fifth of it, and so on.

If we now return to the concords, it is clear that this simple account could have been used to elucidate the idea that the fifth and the fourth are 'superior' to the melodic intervals, and that the octave is superior to them. In the ratio of the octave, the difference and the smaller term are equal. In that of the fifth, the difference is the greatest simple part of the smaller

term, one half, and in the fourth it is the next greatest, one third. The remaining epimorics will follow in order below them. Further, if we grant that the excellence of an interval is reflected in the simplicity, in this sense, of the relation between the difference and the smaller term, the idea can be harnessed to explain the status of the greater concords. The ratio 8:3 is of course not 'simple' in this sense. But when regarded as the product of the octave and the fourth, in the ratios 2:1 and 4:3, the greater term will be constructed from the smaller by first adding an amount equal to the smaller (the 'difference', in the ratio 2:1, being equal to the smaller term), and then adding to the result one third of itself (as the difference between terms in the ratio 4:3 is one third of the smaller term). The processes of increasing a number by an amount equal to itself, and that of increasing a number by one third, have already been treated as suitably simple operations, involving relations that are 'nearer to equality' or which 'divide most nearly into halves' in a relevant sense. In that case the process of performing first one of these operations and then the other may plausibly be regarded as suitably simple too.

Two important questions remain to be answered. First, the phrases to which I drew attention above give very strong evidence that this is the kind of 'closeness to equality' that Ptolemy has had in mind throughout the entire discussion. Why then does he confuse the issue by introducing the quite different sense in which the fourth and the fifth are nearly 'equal halves' of the octave? In broad terms the answer, as I indicated earlier, is probably that he is trying to achieve too many things in a single stroke. The role of these concords as constituents of the octave is important, as is that of the melodic intervals as constituents of the fourth, which is alluded to (16.15–16), but in a less confusing way. (In I.15 Ptolemy will find a role also for the operation of dividing a given ratio into 'nearly equal' melodic ratios. But those which divide it more equally are not for that reason more melodic.)

The introduction of this relation between the lesser concords and the octave is not however irrelevant to the matter in hand. Without it, nothing would be left in Ptolemy's account to mark a categorical distinction between concords and melodic intervals; there would only be a smooth gradient of diminishing excellence from the octave to the melodics. As we noted earlier, Ptolemy deliberately, and with good mathematical reason, adopts the policy of blurring the distinctions between homophones and concords and between concords and melodic intervals. But he cannot dispense with them altogether. The idea that seems to lie behind the apparent confusions of our passage is that these aesthetically accepted distinctions do not genuinely reflect formal features of the various intervals when each is considered by itself; considered in this way there are no

well-marked boundaries between one kind of interval and another. What they reflect are features of their relations with other intervals. The primary concords are the basic constituents of the octave, those into which it can be divided in a mathematically privileged way; and the melodic intervals, as will appear later, are similarly related to the smallest concord. If these are the salient distinctions, they are not, of course, of the kind that the Greeks standardly took them to be; and my earlier comments on this issue will also have been misconceived. There will be no radical distinction in kind between a concord such as the fourth, for instance, and a melodic interval such as the major third, whose ratio is 5:4, or even a major second of 9:8.

In its time, such a view would have seemed outlandish; and at first blush it seems inconsistent with remarks that Ptolemy makes elsewhere. It is the way the interval strikes the ear, quite independently of other considerations, that proves for example that the Pythagoreans are wrong in refusing the octave plus fourth the status of a concord. Nevertheless the idea has something to be said for it. To repeat, there really is no mathematically significant difference in kind between the ratios of the concords and the smaller epimorics. If there is a distinction to be drawn, it will not be grounded in a categorical difference of form. Secondly, it is in fact the case – though if Ptolemy suspected it he could hardly have known it – that the ear's recognition of some intervals as concords and others as discords is conditioned to a great extent by the roles of the intervals in the music with which we are familiar. We ourselves, unlike the Greeks, generally accept thirds and sixths as concordant. I do not think this is due to alterations for better or worse in our auditory apparatus. It is a reflection of the functions these intervals perform in most music commonly heard in our culture between the Renaissance and the present day. Again, after prolonged exposure to the musical language of certain twentieth-century composers, we are likely to construe an interval such as the major second as at least more nearly a concord than we did when we listened only to Mozart. Ptolemy's approach may be nearer the mark than he could reasonably have guessed.[3] Further, if this approach is on the right lines, there will be nothing problematic, after all, in the fact that the Greek ear recognised fourths and fifths as concordant without apparently making any

[3] At 13.10, after asserting that the addition of an octave to a concord preserves the concordance of the relation, Ptolemy adds that the two notes of the octave have the same 'function' (*dunamis*). His association of the relation's acoustic or aesthetic effect with the functions of its notes in accepted melodic systems might be construed as a hint that the former is in some way dependent on the latter. But there is nothing else in the *Harmonics* to support this reading, and if any suggestion of an explanation is intended it is probably the other way round: the similarity of their aesthetic effects explains the notes' identity of function.

comparison, in that act of recognition, between them and other intervals, while perceiving thirds, tones and so on equally immediately as discords. These facts need not entail that concordance and discordance are attributes possessed by intervals independently of their roles in familiar musical practice. The ear makes the distinction readily and immediately, on this account, only because it has become habituated to the roles of these intervals in the musical structures characteristic of its culture.

The second major question concerns the relation between the formal characteristics that Ptolemy has identified in the ratios, and the musical acceptability of the corresponding intervals. A ratio is of the appropriate sort, so Ptolemy asserts, if the difference between its terms is equal to the smaller term or a simple part of it. But what is there to show that it is this that constitutes the formal counterpart of an interval's acceptability to the ear, that it is the very same feature, though represented in a different mode, as that which the ear recognises as making an interval musical? Again, the 'better' ratios are those in which the difference is a greater simple part of the lower term. Why should we agree that the ordering of musical intervals from the finest to the minimally melodic is the same ordering, and that these perceptual and mathematical gradients are the very same one, viewed from different angles?

We should not expect any conclusive proof, at this stage, of the correctness of Ptolemy's *hupotheseis* concerning concords and melodic intervals. The soundness of his approach will not be finally adjudicated until the systems of attunement derived from the *hupotheseis* are submitted, in detail, to the judgement of the ear. But Ptolemy has insisted that the *hupotheseis* must be drawn in some way from the phenomena. This need not mean, as I remarked earlier, that the scientist must actually have formulated his *hupotheseis* as the end product of an orderly enquiry proceeding by some set method from a starting point in perception. He is at least as likely to have come to them in the first instance by quite haphazard and perhaps unconscious trains of association, or by some sort of elimination, after trying out a range of other possibilities, or by any other route whatever. The investigator's intellectual biography is of no interest here. It has no bearing on the question whether his *hupotheseis* are correct, and Ptolemy says nothing to suggest that he had failed to grasp the point. On the other hand, not every idea that occurs to the scientist will be plausible enough to be worth pursuing far. He must be able, on Ptolemy's view, to give reasons for thinking that his *hupotheseis* are on the right lines. Specifically, it should be possible to discern connections of an appropriate sort between the formal characteristics of ratios to which the theorist draws attention, and the perceptible features of the intervals with which these ratios are correlated. There must be something to indicate that the

attribute of ratios which is privileged in the mathematical analysis is in fact the same as the attribute of intervals which is impressionistically grasped and approved of by perception. The 'rational' representation can be conceived as 'drawn from' the impressions of perception only if there are identifiable and apparently relevant affinities between the two.

In the chapters we have been studying, Ptolemy offers no more than hints about the nature of the connections he has in mind; but I believe we can plausibly identify the direction in which they point. The hints are embedded in a few general points about procedure, and in the remarks he makes – either in person or on behalf of those aspects of Pythagorean harmonics which he finds acceptable – about the salient features of the ratios with which the musical intervals are correlated. We can divide them into three interconnected groups.

(i) Equalities

Both the Pythagorean approach and his own begin from the proposition that equal pitches go with equal numbers, and unequals with unequals (11.8–10, 15.18–20). In his own version, the inequalities of pitch manifested in intervals are to be measured and compared (*parametreisthai*) by their closeness to the equalities (15.22–3). The suggestion seems to be that the closer its relation to equality, the more 'musical', in some sense, an interval will be. Further evidence for this interpretation comes from the remarks Ptolemy makes about the octave. According to the Pythagoreans, the octave is the finest interval 'because it is nearest to the equal-pitched' (11.21–3). According to Ptolemy himself, the octave 'creates for the ear the impression of a single note', and other intervals are graded for the closeness of their approximation to this effect (15.10–17); and again, the fact that the octave is 'most unitary' (*henōtikōtaton*, perhaps again 'most like a single note') and finest is treated as a direct reflection of the fact that duple ratio is 'closest to equality' (15.24–7). It seems clear that the nearer the two notes of an interval are to being perceived as musically identical with one another, the better the aesthetic effect of the relation between them will be.

(ii) Simplicity

A relation that is closer to equality in Ptolemy's sense is also one in which the task of comparing the terms is simpler. Epimoric and multiple ratios, according to another Pythagorean view to which Ptolemy subscribes, are better than epimerics 'because of the simplicity of the comparison' (11.15–16); and this idea is unpacked in terms of the 'simplicity' of the

relation between one element of the ratio and the other (11.16–17). In Ptolemy's own account of the matter, this sort of equality is associated also with the 'commensurateness' of elements in the ratio (16.3), a conception that appears again in his account of the approach of Archytas to harmonic divisions (30.13). More specifically, an interval is more melodic in so far as the difference between the terms of its ratio is a 'greater simple part' of the smaller term, or equivalently, in so far as it 'makes divisions more nearly into halves'; and this is linked directly to considerations about equality, since these relations 'are nearer to the equal, just as the half is nearest of all, then the third, and then each of the others in succession' (16.17–21). The criterion of simplicity is applied in all these cases to the comparison of ratios, not of intervals as the ear perceives them. Nevertheless it is drawn in precisely those contexts where Ptolemy is seeking to indicate links between degrees of mathematical excellence in ratios and degrees of perceptible excellence in musical phenomena, and we must treat it as a pointer to his conception of the relation between them. In just one sentence the attribute of simplicity is attached directly to a perceptible interval; the interval of a fifth is more concordant than that of an octave plus a fifth because it is 'simpler and less complex . . . and its concordance is purer' (14.21–3). The 'simpler' interval, then, is the more concordant.

(iii) Degrees of excellence

Ptolemy's reflections plainly depend on his contention that both musical intervals and mathematical ratios can be graded as better or worse, and that the two scales of measurement match one another directly. He appears to have found similar applications of the idea in the work of the Pythagoreans, both in what we called argument A (11.10–17, 20–22) and in their mathematically bizarre attempts to assess degrees of concordance (14.1–15.2). In his own developments of it, as we have seen, the smoothness of the gradient allows no purchase for sharp distinctions between intervals of different classes; but Ptolemy will not abandon these distinctions, and we have found signs that he was looking for criteria of a wholly different kind in order to maintain them (15.3–17, 15.29–16.2). The scale of excellence continues down through the melodic intervals, so that they may be graded as more or less melodic (16.17–18), just as concords may be more or less concordant (14.19–21), and one of the homophones, the octave, is finer than the others (15.26). Intervals that are finer, more concordant and more melodic are associated with ratios that are closer to equality, and admit more simple forms of comparison; more specifically, they are such that the difference between the terms is a greater simple part of the smaller term.

None of this amounts to an adequate account of the way in which we can recognise the perceptible attributes as manifestations of the mathematical ones. But Ptolemy has provided the material for such an account in his general, preliminary reflections in I.1, in a passage to which the language and the conceptual apparatus of I.5–7 unmistakably direct us. He has been discussing the deficiencies from which perception suffers when used alone as a judge of such things as correct geometrical constructions, or the precise values of quantitative differences. He continues as follows.

This sort of deficiency in perceptions does not miss the truth by much when it is simply a question of recognising whether there is or is not a difference between them, nor does it in detecting the amounts by which differing things exceed one another, so long as the amounts in question consist in larger parts of the things to which they belong. But in the case of comparisons concerned with lesser parts the deficiency accumulates and becomes greater, and in these comparisons it is plainly evident, the more so as the things compared have smaller parts. The reason is that the deviation from truth, being very small when taken just once, cannot yet make the accumulation of this small amount perceptible when only a few comparisons have been made, but when more have been made it is obvious and altogether easy to detect. Thus given a straight line it is very easy to construct a smaller or a greater than it by eye, not just because this is a broad sort of distinction, but because it also involves only one comparison. Dividing it in half, too, or doubling it, is still easy, if not to the same extent, since only two comparisons take place. To construct a third of it or to triple it is harder, since in this case three adjustments are made, and it becomes continually and proportionately harder to achieve in the case of things assessed through greater numbers of measuring operations. This is so when we construct the thing we are looking for simply as itself, the seventh, or the seven-times multiple, for instance, and not through easier stages, as when we construct an eighth by first constructing a half, then the half of that, and then again the half of that, or the eight-times multiple by first constructing the double, then the double of that, and then again the double of that. For here it will no longer be the eighth of the one, or its eight-times multiple, that has been grasped, but the halves or doubles of several unequal things. Since similar things occur in relation to sounds and to the hearing, there is needed to help them, just as there is for the eyes, some rational criterion working through appropriate instruments . . . (4.10–5.4)

The analogy between what is said here and the considerations underlying the analysis of the concords is striking and obvious. Comparisons of the sort privileged in the latter passage are simpler, on this account, because they involve fewer steps. Confirming that a given quantity is double the size of another, twice its length, for example, is simple because it requires only two 'comparisons'; we lay the smaller against the greater twice. Triple quantities pose rather harder problems; and so on for the rest. This is patently the same point as reappears in Ptolemy's later comment on melodic intervals: 'those that make divisions most nearly

into halves must be more melodic, . . . as are all those whose differences contain larger simple parts of the things that are exceeded; for these, too, are nearer to the equal, just as the half is nearest of all, then the third, and then each of the others in succession' (16.17–21). It is clear also that comparisons between terms in an epimeric ratio will not, by these criteria, be simple at all. The passage also sheds a little light on one quite particular problem we found, in Ptolemy's treatment of the octave plus fourth. Taken 'simply as itself', the comparison between the terms of its ratio, 8 and 3, would be very difficult to make. But when taken 'in easier stages', each step of the comparison becomes simple, in the sense intended here. To get from 3 to 8 we first double the lower term, and then add to the result one third of itself.

The key points here are first that in I.1 Ptolemy is speaking throughout of judgements made by perception, and secondly that while his examples are drawn from the realm accessible to sight, they are intended as direct analogues of phenomena detected and compared by ear. 'Similar things occur in relation to sounds and to the hearing' (5.2–3). The implication is that in hearing two pitches as forming the concord of a fourth, for example, we are comparing them in a way that is strictly parallel to that in which we judge by eye the relation between two visible lengths. In the auditory case, of course, we do not directly experience what we are doing as a matter of assessing the ratio between two quantities. The situation is more like that in which we perceive by sight the peculiarly satisfactory nature of the relation between certain structural elements in a building or a painting, and perhaps even identify it as one that is satisfactory in a different way from the relation between certain other items. We may not be conscious, in such a case, either of the exact proportions involved or even of the fact that the relations are of a quantitative sort. It clearly remains true, however, that they are indeed quantitative; and it is because the quantities are related in specific proportions that their conjunction is grasped as distinctive and pleasing.

We might reasonably ask why Ptolemy supposes that these presumed auditory comparisons focus on the relation between one of the pitches and the difference between them, rather than directly on the relation between the pitches themselves. But the question has a straightforward answer. In none of the essential cases except that of the octave-ratio, 2:1, can we readily get a clear perspective on the relation between the terms without first identifying the relation in which the terms stand to the difference between them. In the ratio 4:3, for instance, neither term is a 'measure' of the other; but the difference, as in all epimoric ratios, is a measure of each. We acquire an accurate understanding of the relation between the terms by grasping that this difference is a quarter of one term

and a third of the other. Only in this way can the comparison be regarded, in Ptolemy's view of the matter, as a simple one. It is possible that he also thought of this approach as preserving as much as possible of the aesthetic intuition that the interval between two notes is the 'amount' by which they differ. This makes no sense in the context of a theory of ratios if the 'amount' is conceived as an absolute, independent quantity. It must be defined by its relation to the sizes of the ratio's terms. Once again it is the relation in which the pitch-quantities stand to the difference between them that must be the focus of attention.

Ptolemy's position involves one further assumption. The perceptible fineness or beauty of a musical interval corresponds to the simplicity of the quantitative relation implicitly recognised in it by the hearing. This does not mean that an interval's aesthetic excellence is a function of the ease with which we recognise it (though the idea that a particular kind of concord is more easily recognisable than a particular kind of discord was familiar). It means that its excellence rests on a lack of complexity in its formal properties, and that a perceptible counterpart of this attribute is accessible to us through our senses, when we perceive these properties in their alternative, aesthetic guise. It appears to our ears as a qualitative *pathos*, the *pathos* of being a homophone, or a concord, or of being melodic in one of that attribute's varying degrees. The assumption, then, is that formal simplicity amounts to an excellence, and that the same simplicity shines through in the non-quantitative *pathē* we experience. Now the simplicity in question is a closeness to equality, and equality, in this context, is tantamount to identity or unity. Ptolemy's position rests, therefore, on the long-standing theme in Greek philosophy of the superiority of unity over plurality or diversity; and the aesthetic counterpart of this formal attribute is represented in a development of the notion of the 'blending' of pitches which was regularly held to characterise the perceived phenomenon of concordance. Plato, in just the same spirit, had described concordant notes as 'blending together a single experience out of high-pitched and low-pitched movement. Hence,' he continues, 'they provide pleasure to people of poor understanding, and delight to those of good understanding' (*Timaeus* 80b4–6). The intuition for which Ptolemy sought a mathematical explanation had a long history behind it already. If we ask why it is not therefore more pleasurable to listen continuously to one single note than to listen to complex musical sequences, it will be for the same reason that it is less delightful for the mind to focus endlessly on one mathematical unit than to contemplate the beauty of intricate relations. The unity of a single thing has less in it to marvel at than the unity formed by the integration of many diverse elements.

6 Critique of Aristoxenian principles and conclusions

At the end of 1.7 Ptolemy restates the conclusions he has derived from his *hupotheseis*, and explains what his next step will be.

From these points we may say in summary that the first multiple and those measured by it are homophones, that the first two epimorics and those composed from them are concordant, and that those of the epimorics that come after the epitritic [4:3] are melodic. The ratio peculiar to each of the homophones and concords has been stated; and of the melodic class the tone has thus simultaneously been shown to be epogdoic [9:8], because of the difference between the first two epimorics and concords. The ratios of the remainder will receive their appropriate definition in the proper places. But now it would be a good thing to demonstrate the clear truth of those that have already been set out, so that we may have their agreement with perception established beyond dispute, as a basis for discussion. (16.21–31)

There is nothing new about his results, of course. What is important is that they have been shown to follow from *hupotheseis* that are both acceptable to reason and capable of being intelligibly represented as precise, mathematical counterparts of the relevant perceptual impressions. As the final sentence indicates, 1.8 will describe the ways in which the results can be made to display their credentials before the court of perception. Here Ptolemy will use for the first time one of his experimental instruments, the simplest of them, the one-stringed *kanōn* or monochord. As a preliminary, he will explain why it is that stringed instruments are better suited than others for the tasks they are called on to fulfil in harmonic science, and will outline the structure and mode of operation of the one-stringed variety.

Ptolemy's arguments and descriptions in that part of 1.8 raise significant issues; but I shall postpone these until Chapter 10, where I offer a discussion of all the instruments, their credentials and their uses. For the present let us note just one simple point. Ptolemy describes these instruments, and the tests to be conducted, with great care. But no written text can actually constitute such a test; no text can demonstrate that perception accepts or that it rejects the musical credentials of the relations the theorist has described. Readers must either take Ptolemy's

claims about the results of his tests on trust, or else follow for themselves, in practice, the recipes he offers for building and deploying the instruments. It is clear that if Ptolemy meant what he said, he thought that serious students of the science should take the latter course. If we do so ourselves, our ears will have no difficulty with the intervals constructed in 1.7 and tested in 1.8. They are identical with the perfect fourths, fifths and octaves of modern practice. The smaller 'melodic' ratios, and their relations within tetrachords and larger systems, which are to be tested later, are quite another matter. Several of the corresponding intervals sound exceedingly strange, at least to my ears; and it would be absurd for us to rely on our own aesthetic perception to decide whether the sounds of the rationally constructed versions of these melodic intervals are or are not perfect instances of what the musical sensibilities of second-century Alexandria required. We are no longer in a position to submit Ptolemy's harmonic divisions to perceptual judgement of the kind he thought necessary. If we are to take a considered view of their probable accuracy, it must be on some other basis.

We might wonder whether the obstacles to our perceptual assessment of his systems, at a distance of nearly nineteen centuries, are just extreme examples of difficulties that will arise even within a single time-frame and a single culture. Few cultures are monolithic. The assumption that well trained observers within any one culture will agree on what is musically correct is always likely to break down. Ptolemy, however, gives no indication that he sees any difficulty here; and this is not surprising. The idea that the 'facts' about what is musically well formed are culturally determined plays no part in his thought.[1] They are treated as straightforwardly objective; and the faculty through which we make our judgements about them seems to be conceived as a biologically (or perhaps divinely) implanted mode of direct perception, homogeneous in all members of the human race, or varying only by being more or less acutely developed. Nowadays we might argue that a person's response to a musical interval as being correctly or incorrectly formed is not an act of 'direct perception', whatever exactly that may be, but an aesthetic judgement whose roots are at least as firmly planted in experience and culture as they are in

[1] Greek writers on harmonics standardly assume, or insist, that certain patterns of relations are objectively correct, and that their task is to discover which these are. This is the sense, for instance, of Aristoxenus' contention that melody has a 'nature' (*phusis*) of its own (e.g. *El. harm.* 18.5ff, 36.15ff). The fact that some musicians prefer relations which from the scientist's point of view are inferior, or that the intervals actually played on instruments distort the relations that he finds to be correct, reflects only those musicians' degenerate tastes (23.1ff) or the imperfections of the instruments in question (41.24ff), and such tastes and instruments cannot be treated as authoritative in deciding what is and is not genuinely melodic or well attuned.

biological nature. But the problems that provoke these thoughts seem not to arise in connection with the concords. Let us assume for the present that it makes sense to say that Ptolemy's conclusions about them can be 'tested by perception'. Let us assume further that they have been so tested, and have been recognised as correct.

Instead of moving directly to an analysis of melodic intervals, Ptolemy next turns his attention to the claims of a rival school of harmonic theory, that of Aristoxenus and his followers, some of whose views I sketched in Chapter 1. In I.9 he looks critically at the general principles and assumptions inherent in their work, while I.10 and I.11 demonstrate the falsity of specific Aristoxenian propositions about the fourth, fifth and octave. Ptolemy seems to have had three main purposes in launching this attack. At the most general level, the mathematical style of harmonic science, based in ratio theory, found in Aristoxenian harmonics its only serious competitor. It is therefore not surprising that Ptolemy sought to discredit the assumptions and procedures that distinguished it most fundamentally from harmonics in the mathematical tradition. If he succeeded, no plausible rival to mathematical harmonics would remain; and given that he had shown, on other grounds, that all other mathematical approaches were flawed in ways that his was not, his own would emerge as the only form of scientific harmonics that was worth pursuing at all. Secondly, the discussions of detailed conclusions in I.10 and I.11 are designed to persuade us that Ptolemy's objections are not only justified by theoretical considerations at a high level of abstraction (against which his rivals might legitimately advance their own theoretical counter attack); they are squarely grounded in evidence of a sort that Aristoxenians were ideologically committed to accepting, that of perception itself. (Here, as noted above, the actual tests and their perceived results will be crucial, and their description in the text is not by itself sufficient.) Finally, since the Aristoxenians claimed for their views the authority of perception, and since Ptolemy also insists on submitting his own conclusions to perceptual tests, his assertion that what perception reveals is inconsistent with Aristoxenian opinions cannot stand alone. It needs to be supplemented with a more general account of the ways in which their application of perceptual criteria went wrong.

Ptolemy begins by contrasting the kinds of error into which the Pythagoreans had fallen with those perpetrated by this rival school. 'We should not find fault with the Pythagoreans in the matter of the discovery of the ratios of the concords, for here they are right, but in that of the investigation of the causes, which has led them astray from the objective; but we should find fault with the Aristoxenians, since they neither accepted these ratios as clearly established, nor, if they really lacked

confidence in them, did they seek more satisfactory ones – assuming that they were genuinely committed to the theoretical study of music' (19.16–20.2).

The Pythagoreans' quantitative results were correct, then, though their explanatory *hupotheseis* were not. (Ptolemy's low opinion of their scientific credentials is thus a clear indication of the importance he attaches to correct explanation, over and above the correct description of the phenomena.) The Aristoxenians rejected these results, Ptolemy alleges, but offered none of their own to replace them, or none that he will recognise as being of the right sort. As a result, so his contemptuous closing aside suggests, he can hardly bring himself to take their theoretical pronouncements seriously.[2] These initial comments, vague as they are, are specifically directed to Aristoxenian views about the concords. But Ptolemy immediately broadens the scope of his attack to encompass the whole of the Aristoxenian approach to quantification. I shall not follow exactly the order of his exposition, since in his opening salvo several issues are intertwined. Three distinct lines of criticism emerge in the sequel, and I shall discuss them separately, though there remain important connections between them.

(i) Empty Aristoxenian spaces

Let us begin with the most fundamental, which in its detailed version comes last in Ptolemy's text.

In general, it would seem an absurdity to think that the differences possess a ratio that is not exhibited through the magnitudes that make the differences, and to suppose that the magnitudes have none – the magnitudes from which it is possible immediately to derive the ratio of the differences. And if they were to deny that their comparisons are of the differences between the notes, they would be unable to say of what other things they are. For the concordant or the melodic is not just some empty distance or mere length, nor is it bodily and predicated of one single thing, the magnitude. Rather, it is predicated of two things at least, these being unequal – that is, the sounds that make them – so that it is not possible to say that the comparisons in respect of quantity are of anything but the notes and the differences between them, neither of which do they make known or provide with a common definition, a definition, that is, which is one and the same, and through which it is shown how the sounds are related both to one another and to the difference between them. (21.9–20)

Ptolemy's line of thought here does not leap to the eye, but it can be clarified and supplemented by some remarks made earlier in the chapter.

[2] Aristoxenus, on the other side of the methodological fence, is equally contemptuous of the tradition to which Ptolemy more nearly belongs, treating mathematical harmonics as a negligible irrelevance (*El. harm.* 32.18–28).

For they must necessarily agree that such experiences come to the hearing from a relation that the notes have to one another . . . Yet in what relation, for each species [of concord], the two notes that make it stand, they neither say nor enquire, but as if the notes themselves were bodiless and what lie between them were bodies, they compare only the intervals [or 'distances', *diastaseis*] belonging to the species, so as to appear to be doing something with number and reason. But the truth is precisely the opposite. (20.2–9)

The opening sentence of the first of these passages is better designed for rhetorical effect than for argumentative cogency. Of course what it says is true. It would be absurd to treat the difference between two items as a ratio between two quantities, and simultaneously to deny that the items compared have any quantitative attributes. But this has no force against the position of the Aristoxenians, who flatly rejected the whole practice of representing intervals as ratios. On their view the two items, the notes, by which any interval is defined, do indeed lack quantitative attributes. So far as their pitches are concerned, they appear in Aristoxenian theory as dimensionless points on a linear continuum, separated by distances (*diastaseis*) or intervals (*diastēmata*) which are identified by their sizes (*megethē*), as tones, fourths, fifths and so on. An interval is not a ratio between two quantities, but a single quantity, a measurable distance between two points in the dimension of pitch. The approach was justified by its supposed correspondence to the way in which relations between pitches present themselves to the hearing. The idea is that we do not hear notes that differ in pitch as items differing in quantity or number, but only as sounds placed at different points in auditory 'space'.[3] According to this account then, as Ptolemy says, a note's pitch is not a measurable attribute of the note. It is not in fact an attribute of the note at all, only the 'place' where the note is located. What has quantity and can be measured is the distance between the members of any pair of such places or points.

For us, I think, there is nothing very recondite or obscure in this general conception. It corresponds rather closely to the ways in which relations between pitches are still standardly described in our language and represented in our notation. Ptolemy does not deny that the Aristoxenian notions of pitch-points and intervening distances are embedded in everyday ways of thinking and speaking, though in fact he might have done; the Greek language very rarely employed the metaphors of 'high' and 'low', 'up' and 'down' in connection with pitch; and there was nothing in the vocabulary regularly used in this connection, or in Greek systems of notation, to encourage the notion of pitch as a quasi-

[3] See e.g. Aristox. *El. harm.* 8.13–921, 15.15–16, cf. 3.21–4.

spatial dimension.[4] But he does deny that such conceptions make sense, if taken seriously. His fundamental thesis is that we never perceive 'distances' in the dimension of pitch simply as such. What we perceive is a pair of notes and the relation in which their attributes stand to one another. More specifically, the attributes of being concordant or being melodic are relational. They are not one-place predicates of 'distances'. No one thing, be it a note or a distance, can be concordant in its own right, but only in relation to something else. In any case, such attributes could not possibly belong to 'empty distances', since these distances, as such, could have no sonorous properties and could make no impression on the hearing (21.13–14). Nor is such a distance coherently conceived as the length of some body, whose 'size' could be measured as one single thing, since there is no body for it to belong to (21.14–15). When we do what we call 'hearing an interval', the only things actually heard are the two sounds. The perception of the interval can then only be our grasp of the relation in which certain attributes of these sounds stand to one another, since nothing else is presented to our experience. And in that case the sounds must have perceptible attributes which constitute their pitches. An interval is not an attribute of anything, but a relation between pitches; and a pitch cannot be a featureless point in some 'space', the 'place' where some sound is, but must be a perceptible attribute of the sound itself.

An Aristoxenian might reply that though some of this is true, the argument is misleading. Our perception of distances between points of pitch is in its essentials exactly parallel to our perception of the space inhabited by visible things. We do not see empty space or distance as such. All that we see are things, separated from one another within this space. Their spatial positions and relations are not attributes of them, but relations between them; nevertheless we can grasp, by sight, the sizes of distances between items in this dimension. Similarly, we can grasp through hearing the sizes

[4] Where our staff notation presents a picture of spatial relations between 'higher' and 'lower' notes, Greek notation used symbols based on letters of the alphabet, and suggested nothing of the sort. The prevailing metaphors embedded in words used to describe pitch were also non-spatial (sharp and heavy, tense and relaxed; the metaphors of 'above' and 'below', *anō* and *katō*, do appear to be drawn on in the relevant senses at [Ar.] *Problems* XIX.37 and 47, but the usage is altogether exceptional). Names assigned to the notes of the Greek system derived such spatial connotations as they have from the positions of strings on an instrument, not from conceptions of pitch. We may note in passing that even Aristoxenus' quasi-spatial imagery never suggests that pitches differ by being 'higher' or 'lower'; they are apparently conceived as differing merely in their 'distances' from some point of origin. A passage in Porphyry (*Comm.* 95.13–19) offers us the engaging picture of Aristoxenian professors conveying the notion of these 'distances' to their students by pacing out greater or smaller distances horizontally, not by gesturing at different levels in a vertical dimension.

of the 'gaps' between sounds, even though it is only the sounds that are heard. But this response is inadequate. A single item in ordinary space will look exactly the same, other things being equal, no matter where it is located. It is no doubt true that because of progressive wear and tear, or subtle local variations in the quality of the light, my much-enduring copy of Ptolemy's *Harmonics* presented a slightly different appearance when I consulted it in the South Island of New Zealand from the ones it has offered me in Warwickshire, Rome, Paris, Toronto, Brisbane and wherever else it has travelled in my baggage. But it is not *because* it looks different, if it does, that I know that it is in a different place. By contrast, we do know that one sound is in a different pitch-location from another precisely because, in a specific respect, the two sound different. The pitch is given as one of the sound's perceived attributes, and cannot change without audible alteration in the sound itself.[5] Pitch, then, is an audible property of sounds in a way in which spatial location is not a visible property of material bodies. Nor is the interval between two sounds something that we can perceptually assess except by attending to the audible properties of each, and the relation between them. It is not given, independently of these properties, in some special act of identifying their quasi-spatial locations and measuring the distance by which they are separated. This has surely been too slight a study of a complex matter; but I conclude, provisionally, that at this general level Ptolemy's criticism of the Aristoxenians is well founded, if not very clearly expressed. Let us pass on to the second and third, which attack narrower aspects of the Aristoxenian view.

(ii) Defining intervals

'In the first place,' says Ptolemy, 'they do not define in this way what each of the species [of interval] is in itself – as when people ask what a tone is, and we say that it is the difference between two notes that comprise an epogdoic ratio [9:8]. Instead, there is an immediate shift to yet another undefined term, as when they say that the tone is the difference between [the intervals of] the fourth and the fifth' (20.9–14). This argument is then developed in two ways. First, the tone is defined, on the approach attributed to the Aristoxenians, by its relation to two other intervals. And yet, Ptolemy asserts, a tone can be accurately constructed by perceptual means 'simply as such', without recourse to that relation (20.14–18). The remark is surprising. It seems to mean that the interval of a tone can be constructed, and presumably identified, accurately and independently of

[5] Cf. Theophrastus fr. 716 (Fortenbaugh), lines 29ff, 108ff.

others by ear alone, that a tone is something our ears can be relied on to recognise directly. If that is what Ptolemy means, it will give him a plausible move in the argument, the idea being that the description 'the difference between the intervals of a fifth and a fourth' cannot capture what the tone essentially *is*, if its essential characteristics can be recognised and constructed without reference to those intervals. But it is far from clear that this proposition is one that Ptolemy himself would normally endorse.[6] Perhaps he should be understood as stating it not in his own voice but on behalf of his opponents, though I see no hint in the text that this is his intention. He could indeed have had plausible grounds for attributing such a thesis to them.[7] In that case he will not be trying to show that their definition of the tone flies in the face of the facts, but that in offering it they are being inconsistent – their way of defining the tone is undermined by other assumptions to which they are committed.

In either case the point must be that the definition inevitably fails to capture the nature of the interval, if it is granted that our correct construction and identification of it do not depend on our grasping its relation to the items by reference to which it is defined. This argument seems inconclusive. It is true that within the largely 'essentialist' framework that Ptolemy presupposes, a definition designed to capture the 'essence' of something will not succeed if we can identify that thing, with absolute assurance, by characteristics entirely independent of those mentioned in the definition. If those independent characteristics are essential to it, and if the definition neither states nor entails that fact, then it is at best a partial definition. Clearly, however, this conclusion will not follow if the characteristics by which we recognise the thing are not, after all, independent of those mentioned in the definition, but are aspects or manifestations of them. If we could reliably recognise a semicircle when we saw one, quite directly, by the characteristic 'look' of its immediate appearance and without having first to construct (mentally or in practice) the complete circle of which it is half, that would by no means be enough to show that the strategy of defining the semicircle by reference to the complete circle is wrong. It would have been shown to be wrong only if we knew that the attributes giving it that characteristic 'look' are altogether independent of its relation to a complete circle. Propositions of that sort may be very hard to confirm, not least because we may be quite unsure of

6 See the general argument at 4.19–5.10; more specifically, 24.1–8, and the dilemma offered to the Aristoxenians at the beginning of I.11.

7 See particularly Aristox. *El. rhythm* II.21; but although Aristoxenus measures other intervals by reference to the tone, he typically treats the tone itself as determinable only indirectly, by reference to relations between concords (especially *El. harm.* 55.3ff, and e.g. 45.34–46.1).

what, exactly, the features are that we rely on when we recognise the thing 'directly', and how they should be defined. In that case the mere fact – if it is one – that the tone is immediately recognisable by perception is by itself no proof that a definition referring to the fifth and the fourth is misconceived; and the fact that an Aristoxenian theorist would suppose that it is so recognisable, if indeed he would (Aristoxenus himself at least sometimes did not; see n. 7 above), need not commit him to a view with which the definition is inconsistent.

In the second phase of this argument, Ptolemy contends that the Aristoxenian definitions are uninformative because circular. The tone is defined as the difference between the fourth and the fifth. What then is the size of this difference? The only reply these theorists can offer, Ptolemy says, is to the effect that 'it is two of those [distances] of which the fourth is five, and that this again is five of those of which the octave is twelve, and similarly for the rest, until they come back round to saying ". . . of which the tone is two"' (20.18–22). Though what is said here is true, it again does not create serious difficulties for the Aristoxenian position. All it entails is that they cannot express the size of any interval except by reference to the sizes of others. There need be nothing perniciously 'relativistic' about this style of quantification, so long as we accept two other fairly plausible propositions: first, that intervals of certain sizes are such that their identity can be recognised directly (though of course if we want to *express* their sizes, we must do so in terms of their relation to other intervals); and secondly that the sizes of all other intervals can be measured by the relations in which they stand to those ones. According to Aristoxenus, concords are directly recognisable, and the tone is to be defined by reference to concords; and the sizes of all musical intervals, including the concords, can be expressed in terms of their relations to the tone. There is nothing particularly troublesome about the fact that the sizes of the intervals presented to our ears cannot be represented as functions of measurable quantities of some other sort. Much the same is true, after all, of linear distances in ordinary space.

(iii) Ratios, intervals and lengths on a string

Ptolemy has one further argument. It is curious, to say the least. He presents it as a continuation of his previous line of criticism, a development of the thesis that the Aristoxenians have no adequate way of defining the sizes of intervals. Their definitions of these 'differences' are inadequate, he says, for the additional reason that 'they do not relate them to the things to which they belong (*toutois hōn eisin*); for there will turn out to be infinitely many of them in each ratio if the things that make them

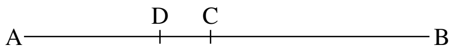


Fig. 6.01

(*tōn poiountōn autas*) are not defined first' (20.23–5). These introductory remarks are enigmatic, partly because of the obscurity of the references of the phrases I have transliterated. The things that should have been defined first, and have not been, are probably most naturally understood as being the notes themselves, since we have seen Ptolemy insisting that the things that 'make' or constitute an interval are the notes themselves, complete with their essential quantitative attributes. The interval is 'made' by the relation between them. Alternatively, Ptolemy may still be thinking about the idea explored in the previous paragraph, that the Aristoxenians cannot give an adequate independent characterisation of the item that they are measuring, the interval or distance itself, but can only say that a fourth is five of the units of which a tone is two, and so on. But in either case the sense of the conclusion Ptolemy announces will be the same, that to each of the ratios (by which, in Ptolemy's style of harmonics, any given interval is defined) there will correspond not just one Aristoxenian 'distance', but an infinite number of them. How can he make this extraordinary conclusion stick?

I broke off the quotation above in mid-sentence. There will turn out to be infinitely many 'differences' corresponding to each ratio, it continues, 'if the things that make them are not defined first, just as in instrument-making not even the distances that make the octave, for instance, are kept the same, but at the higher pitches they are made shorter. Thus if you compare with one another equal concords based on different boundaries, the length (*diastasis*) of the difference will not be equal in all cases, but if one attunes one to another of those of the notes that are higher it will be smaller, and if one attunes those that are lower it will be greater. For if we assume that the distance AB is an octave, A being thought of as the higher limit, and take two fifths, one downwards from A – call it AC – and the other upwards from B – call it BD – the distance AC will be smaller than BD because it falls between higher pitches, and the difference BC will be greater than AD' (20.25–21.8).

The construction is awkwardly described; and whether it is genuinely Ptolemaic or not, the diagram that accompanies it in the manuscripts (Figure 6.01) is unhelpful. In particular, the 'assumption' that AB is an octave is, on the face of it, nonsensical. It is clear from Ptolemy's remarks about instrument-making that when he speaks here of smaller and greater distances, these distances are lengths of such things as strings or pipes.

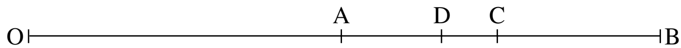


Fig. 6.02

Now Ptolemy says explicitly that AB is a distance; and in that case A and B must be the points that are its boundaries. But where lengths or distances are lengths of string or pipe, any one length, such as AB, must represent a note, not an interval. Similarly, a point on the string cannot be a note; hence A cannot be a note, and cannot in that sense be the upper limit of an octave. It looks as if Ptolemy has been distinctly careless in his manner of expression. As far as I can see, the only way of making sense of what he says is to suppose that when he tells us that AB is an octave, and A its higher limit, he means that the length of string from its point of origin to A yields the note an octave higher than that given by the length of string from its point of origin to B. If we call this point of origin O, the appropriate diagram will be the one given in Figure 6.02.

We have been told to construct two fifths, one downwards from the note at the top of the octave (OA), and one upwards from the lowest note of the octave (OB). The length OC corresponds to the pitch a fifth below OA; it is constructed by increasing OA in the ratio 3:2. Since OB:OA is 2:1, and OA is therefore half OB, point C will fall halfway between A and B. The length OD sounds a fifth above OB, and is therefore two thirds of OB (since OB:OD = 3:2). Hence point D is only one third of the way along the string from A to B. It will follow, as Ptolemy says, that the distance AC, supposedly treated as representing the fifth by which OC differs from OA, will be smaller than BD, which allegedly represents the fifth by which OD differs from OB. What Ptolemy calls the 'differences', BC and AD, are the distances corresponding respectively to the difference between OB and OC, and the difference between OA and OD. The musical intervals between OB and OC, and between OA and OD, will both be fourths, but the corresponding 'differences' between the relevant lengths of string will again not be the same.

But as a critique of Aristoxenus and his followers, the argument is bewilderingly absurd. The propositions about the relations between lengths are of course true; but they have nothing to do with Aristoxenian theory. On an Aristoxenian approach, the 'distance' between notes a fifth apart, for instance, remains the same irrespective of the absolute pitches of the notes, just as Ptolemy's ratios are unaffected by the pitch-range in which the notes occur. Neither Aristoxenus nor any of his successors had the slightest intention of identifying these distances with lengths on a string. They were distances in a purely auditory dimension that had no

visible counterpart; and from this perspective Ptolemy's demonstration is simply beside the point. It would become relevant only if he could show that the Aristoxenians can be compelled, by argument, to 'translate' their propositions about the sizes of intervals into parallel propositions about lengths on a string. But of course he cannot, and any argument that purported to produce this result would be the merest conjuring trick. It is hard to resist the conclusion that when he formulated these criticisms of the Aristoxenian position, Ptolemy was either seriously confused, or joking, or guilty of deliberately dishonest dealing.

There is a slightly more charitable interpretation that we might try to offer him. The argument is prefaced with the phrase 'Just as in instrument-making . . .' (20.25–6), and the phrase might reasonably be held to govern the whole of what follows. In that case the argument might not be an attempted proof that the Aristoxenian view is incoherent (if it were, it would fail); it might be designed merely to exemplify the kind of difficulty that can arise if we try to correlate the 'size' of an interval with any genuinely measurable distance, such as a length of string. As an argumentative move, this would be relatively weak. It would not show that there is no intelligible way of conceiving the 'distances' the Aristoxenians have in mind, only that this particular suggestion breaks down. At best it would sharpen the edge of Ptolemy's challenge to them to produce a coherent explanation. But the move would at least make sense.

Unfortunately for this charitable view, or any other, there are indications elsewhere that Ptolemy took seriously the notion that Aristoxenian differences of pitch can be directly matched against differences of length on a string. Towards the end of Book II he sets out to represent Aristoxenus' harmonic divisions, along with others, in such a way that their intervals can be reproduced accurately on the strings of an experimental instrument; and the strategy he uses involves precisely the same misunderstandings as I attributed to him in my first interpretation here. (The crucial passage is in II.13, at 69.29–70.3, with the tables giving Aristoxenian divisions in II.14.) The problems raised by the later passage will be discussed in Chapter 11 below. For present purposes the most important conclusion arising from that discussion will be that though the confusions cannot be wished away, they are probably not of Ptolemy's own making; he seems to have taken them over wholesale from an earlier source. I would reckon it rather more likely that he borrowed carelessly than that he did so with intent to deceive. That is evidently not something I can prove; it will depend, at least partly, on one's overall impression of the degree of good faith with which Ptolemy has approached the whole subject, and we are not yet in a position to make judgements about that. But it is obvious that these passages are going to pose awkward problems

for scholars who would like to credit Ptolemy both with consistently high standards of reasoning and with unswerving dedication to sincerity and truth.

This completes Ptolemy's attack on the more general features of Aristoxenian procedure. If my assessments are correct, the third of the arguments we have reviewed fails outright, and the second is not particularly damaging. The first and most general, however, deals a well-aimed and significant blow. At the least, it will require an Aristoxenian to review his position from the ground up, and no Greek theorist seems to have found an adequate response. We turn now to arguments directed against one special group of Aristoxenian propositions, those to do with the concords; but we shall find that they too raise important issues of a wider sort.

The argument of 1.10 challenges the proposition that the concord of a fourth consists of exactly two and a half tones.[8] On Ptolemy's approach and that of the mathematical theorists in general, this cannot be true. If, as the Aristoxenians agree, the tone is the difference between a fifth and a fourth,[9] and if the ratios of these concords are 3:2 and 4:3 respectively, then the ratio of the tone is 9:8. But the ratio of the interval by which the fourth exceeds two such tones is not that of a half-tone. It is in fact 256:243 (that is, $4{:}3 = 9{:}8 \times 9{:}8 \times 256{:}243$). Now the ratio of the half-tone, A:B, would be such that $A{:}B \times A{:}B = 9{:}8$, and as the Archytan theorem mentioned earlier (pp. 65–6) had shown, A:B cannot be a ratio of integers. (Ptolemy alludes to the theorem at 24.10–11, though only in passing.) Hence, of course, it cannot be 256:243. In fact this latter ratio specifies an interval slightly smaller than the half-tone, since $256{:}243 \times 256{:}243$ is a little less than 9:8. Ptolemy offers an arithmetical working of the details involved at 22.17–23.18.

He prefaces this elementary demonstration with an account of the argument given by Aristoxenians for their view, that is, for the view that the fourth is exactly two and a half tones. It corresponds quite closely to what is presumably the original version of the argument, which is set out in the surviving work of Aristoxenus himself (*El. harm.* 56.13–58.5), though there are differences to which I shall draw attention shortly. It requires us to construct, on an instrument, a sequence of intervals of various sizes, attuning them by ear, and to attend carefully to the relations between the resulting pitches. Since the intervals are constructed and assessed entirely by ear, it does not matter whether the strings' pitches are adjusted by changing their tension, as on a lyre, or by altering their sounding-length, as on a monochord, or by some other method. In

[8] This is stated tentatively by Aristoxenus at *El. harm.* 24.4–10, confidently at 46.1–2, and an argument to confirm it is supplied at 56.13ff. A marginal qualification or caveat is suggested at 55.3–6.

[9] E.g. Aristox. *El. harm.* 45.34–46.1.

particular, there is no need for the apparatus to allow measurements to be made, for instance of length, through which audible intervals could be correlated with ratios of numbers. In the Greek context, the best instrument for the purpose would not be the monochord (which Aristoxenus, in any case, is unlikely to have used), but an instrument with several strings, an ordinary lyre or kithara, for example, so long as its tuning devices were good enough to permit the rather sensitive adjustments that are needed. The procedure described in the texts of Aristoxenus and Ptolemy is in essence straightforward, but the barrage of detail can be confusing. To simplify presentation, I suggest that we consider the construction in terms of the notes of a modern keyboard, with the one important qualification that we are to imagine ourselves not only as playing them in the order prescribed, but as having to tune each of them very carefully and accurately, by ear, to the necessary intervals as we go along.

Ptolemy's version of the procedure can be paraphrased as follows. Begin from B♭ and attune a fourth upwards, to E♭. Now construct a ditone upwards from B♭, giving D, and a ditone downwards from E♭, giving B. It follows that the intervals B♭ – B and D – E♭ are equal, and that each constitutes the interval we are interested in, the remainder of a fourth when a ditone is subtracted. We next construct a fourth upwards from B, giving E, and a fourth downwards from D, giving A. Then since B♭ – E♭ and A – D have both been constructed as fourths, A must lie at the same interval below B♭ as does D below E♭ (that is, A – B♭ and D – E♭ are equal); and since B♭ – E♭ and B – E are also equal fourths, B♭ – B and E♭ – E must be equal. Since it has already been shown that B♭ – B equals D – E♭, it follows that all four of these intervals, A – B♭, B♭ – B, D – E♭ and E♭ – E, are the same. But now, Ptolemy continues, the Aristoxenians assert that the whole of A – E is the concord of a fifth, which is greater than the fourth by a tone. In that case, since B♭ – E♭ is a fourth, the difference between B♭ – E♭ and A – E is a tone. This difference is made up of the two equal intervals, A – B♭ and E♭ – E. Hence each of these is a half-tone. But it was shown that D – E♭ is equal to E♭ – E. Then the difference between the fourth B♭ – E♭ and the ditone B♭ – D, which is D – E♭, is also a half-tone, and the fourth has been shown to consist of exactly two tones and a half.

The conclusion conflicts, as we have seen, with the one entailed by the calculations of mathematical theorists, including Ptolemy. On one side or the other something must apparently have gone wrong. We should not diagnose the dispute as evidence of a 'battle' between reason and perception, Ptolemy insists (implying, perhaps, that it had been so diagnosed in the past). It is really due to 'the things that have been incorrectly hypothesised (*tōn diaphorōs hupotithemenōn*)' by the Aristoxenians (23.19–20). The point is clarified a few lines later. It is not that the Aristoxenians are

wrong in adopting perception as a criterion. The trouble is that 'in cases where perception is naturally competent to judge, that is, in respect of the greater distinctions, they are altogether distrustful of it, while in those where it is not by itself sufficient, that is, in cases where the differences are less, they trust it, or rather, they add on to the primary and more authoritative judgements others that are opposed to them' (24.4–8).

This contrast between perception's competence in respect of the 'greater distinctions', and its relative incompetence as a judge of those where 'the differences are less', points back to the thesis of I.1, which we drew on in Chapter 5, that perception is tolerably reliable 'in detecting the amounts by which differing things exceed one another, so long as the amounts in question consist in larger parts of the things to which they belong' (4.11–13); but that as these amounts become smaller fractions of the things compared, so perception becomes less accurate in its judgement of them (4.13–5.3). In the present case, Ptolemy claims, the Aristoxenians reject the evidence of perception when it 'virtually shrieks its clear and unmistakable recognition' of the fifth and the fourth in cases where they are constructed according to their proper ratios; if they accepted that evidence, they would be bound also to accept its mathematical consequence, that the fourth is less than two and a half tones (23.21–24.4). Yet in each of these cases the difference between the terms amounts to a large part of the things compared, being a half of the smaller term in the case of the fifth and one third in that of the fourth. While rejecting these pieces of perceptual evidence, the Aristoxenians are said to accept others which are much less reliable, and which are 'opposed to those that are primary and more authoritative', opposed, that is, to those by which the credentials of correctly constructed fourths and fifths are unambiguously recognised.

It is not clear that Ptolemy has chosen quite the right line of attack. He points out (24.8–19) that the difference between the *leimma* of 256:243 (the residue of a fourth after two whole tones) and the half-tone is much too small to be picked up reliably by the ear. But as he remarks himself, 'that so slight a variation is capable of being judged by the hearing not even they [the Aristoxenians] would say' (24.20–1); and they need make no such claim. Nothing in their procedure requires us to be able to detect this distinction. At most, Ptolemy's point shows only that we could not tell by ear whether a given interval, taken in isolation, is a *leimma* or a half-tone, and this is not relevant, since at no stage of the construction are we required to make this judgement.

Nor does Ptolemy's claim about perception's greater competence in judging intervals where the 'differences' are larger parts of the terms have as direct a bearing on the matter as he seems to suppose. He develops the

point as follows. 'If it is possible for perception to mis-hear something of this size [the *leimma* or the half-tone] in one instance, it must be much more possible in the addition of several instances, something that their proposed demonstration involves them in, with the fourth being taken three times and the ditone twice, in different positions' (24.21–5). But if we assume, with Ptolemy, that the ear is competent to assess what purport to be instances of the concord of the fourth, then the three fourths in the construction should pose no problems. The ditones might well seem more troublesome. As Ptolemy says, a ditone (whose ratio is 81:64) will be hard to construct by ear even once, according to his criteria. Even the tone would be easier, since its ratio is 9:8, and 'for perception the more commensurate intervals are the more easily grasped' (24.25–9). But this is misleading. Aristoxenus agrees that such intervals as the ditone cannot reliably be constructed, just by themselves, by ear alone. Hence he offers a way of constructing them by means of moves through nothing but concordant intervals, that is, through fourths and fifths, which on both Aristoxenus' view and Ptolemy's are capable of being reliably assessed by ear (*El. harm.* 55.13–23). If the ditones are constructed in this way, nothing in Aristoxenus' procedure will depend on the ear's judgement of intervals that are not concords; and all the intervals assessed by ear will conform to Ptolemy's condition that the differences between the terms must be large integral parts of the terms. Hence the contention that the ear is unreliable in discriminating lesser 'differences' is beside the point.

Though Ptolemy has made things easier for himself by suppressing Aristoxenus' account of the construction of ditones,[10] and though the angle of his attack seems in certain respects ill judged, nevertheless he is right in his conclusions, and he has already deployed the resources needed to support them satisfactorily. The point is not that the ear judges fourths and fifths more reliably than intervals in lesser ratios. It is that while it recognises the perfection of such intervals when they are rationally constructed, and can detect the imperfection of mere approximations when it compares them with the genuine article, it may none the less accept as perfect one of these approximations when confronted with it alone (3.20–4.7). The concords of Aristoxenus' procedure are attuned purely by ear, and no comparison is made with those constructed according to the ratios dictated by reason. Hence none of them is reliable. If the intervals really conformed to the appropriate ratios, it would turn out that

[10] In view of the close similarities between Ptolemy's account of the Aristoxenian procedure and its original in *El. harm.*, his silence on this point is unlikely to be due merely to ignorance. But this discrepancy, and another that I shall mention shortly, might be deemed sufficient to suggest that he did not, after all, have the text of *El. harm.* in front of him as he wrote; just possibly he is relying on an inadequate intermediary source.

though the crucial intervals (the 'semitones' rising from A, B♭, D and E♭) are indeed all equal, they are not true half-tones but *leimmata*, intervals smaller than the half-tone. In that case the whole interval, as constructed, from A to E will amount to the ditone B♭ – D plus three *leimmata*. A ditone plus one *leimma* is a fourth; but the remaining two *leimmata* together are not as large as a tone (that is, $256{:}243 \times 256{:}243 < 9{:}8$). Since the concord of a fifth is the sum of a fourth and a tone, the interval A – E built up according to the Aristoxenian recipe cannot be a genuine fifth. That is where the real problem lies, and here Ptolemy's remarks about the ear's incapacity to distinguish between minutely different intervals do indeed become relevant. The ratio of the interval A – E, if it is built up through Aristoxenian procedures by mathematically accurate steps, will differ from that of the true perfect fifth only to the extent that 9:8 differs from $256{:}243 \times 256{:}243$, which is only by about one part in 65.

We should notice, however, that though most Aristoxenians seem to have accepted without qualms the equation of the fourth with two and a half tones, and of the fifth with three and a half, and though Aristoxenus usually proceeds as if these equations were correct (for instance in the theorems of *El. harm.* III), he does not quite claim that the construction we have been considering proves them. The relevant sentence of his presentation of it runs as follows. 'When this construction has been set up, we must bring to the judgement of perception the outermost of the notes that have been located. If they appear to perception as discordant, it will be evident that the fourth is not two and a half tones; but if they sound the concord of a fifth, it will be evident that the fourth is two and a half tones' (*El. harm.* 56.33–57.3). Aristoxenus does not suppose, then, that the question whether the interval between the extreme notes is a fifth is settled by the form of the construction itself. Perception must be the judge, and what he says about it allows for the possibility that the 'Aristoxenian' thesis is mistaken. No doubt he believed that it is in fact true. But if Ptolemy had the passage in front of him as he wrote (and his version is in most respects very close to the original), he is plainly misrepresenting it, much as he is in his references to the construction of ditones.

Yet Ptolemy had no need for sleight of hand. A perfectly straightforward strategy was open to him. Let the Aristoxenian pursue his procedure as described, while ensuring that each of the fourths constructed is assessed in the way which really does allow us, so Ptolemy holds, to distinguish the genuine from the spurious by ear. The consequence will be that each of these fourths will in fact be an interval in the ratio 4:3. Once all the notes have been found, let him construct the concord of a fifth upwards from the lowest note, by the same reliable method. This fifth will then be an interval in the ratio 3:2. It will turn out (as can easily be shown by

calculation) that the upper note of this fifth is not identical with the highest note of the original construction, and it will be clear to perception that it is the former, not the latter, which is genuinely at the interval of a fifth above the lowest note. Our hearing will 'recognise the more accurate as legitimate, as it were, beside the bastardy of the other' (4.6–7).

In short, if Aristoxenus had carried out his own recommendations in the proper manner, he could not have remained an 'Aristoxenian', for it is not only the special question about the size of the fourth that is at issue here. It will be a consequence of these conclusions about the fourth that we have no way of representing its size in terms of the tone and its fractions, except by saying that it is a little less than two and a half tones; and we shall have exactly the same difficulty with the fifth and the octave. This will leave no basis at all for identifying as half-tones, thirds of tones, and so on, the small intervals constituting individual steps in an attunement. The only acceptable form of measurement will be in terms of ratios; and if that is granted, a great deal that is characteristic of Aristoxenian harmonics will begin to crumble. Some more recent theorists, Ptolemy remarks in passing, 'employ a combination [of premisses] based on both sets of criteria' (23.20–1). This can be interpreted in various ways; but one thing it is likely to mean is that they tried to combine the representation of pitch-relations as ratios, as in mathematical harmonics, with Aristoxenian analyses of acceptable forms of attunement, melodic successions of intervals, and so on. We do indeed find eclectic procedures of this sort in a number of writers roughly contemporary with Ptolemy or a little earlier, notably in Theon of Smyrna (or his sources) and in Nicomachus. Later we meet them again in Aristides Quintilianus. The consequences, inevitably, are inconsistency and confusion.

Our discussion of I.10 shows that Ptolemy has a strong case against the Aristoxenians, even though he seems not always to have made the best use of his ammunition. By referring us back once again to the ideas of I.1, the passage serves two further purposes, additional to those I identified at the beginning of this chapter. First, it underlines the consistency with which Ptolemy's own ways of calling on reason and perception are developed out of his initial reflections. Secondly and more specifically, it reminds us that when properly employed, these faculties are comrades, not rivals. The appearance of conflict arises only out of their misuse. Reason relies on perception to identify, rather roughly, those relations that are musically acceptable. Through reflection on these data it arrives at *hupotheseis* about the rational principles to which perfect instances of such relations must conform. Examples formed accurately in accordance with the *hupotheseis* are then submitted to perception, and if they are indeed perfectly formed, the ear will unfailingly recognise their superiority to those

that are not. It is only when perception tries to go to work on its own, as in the procedures of the Aristoxenians, or when theoretical *hupotheseis* are adopted without due attention to the judgement of the ear, as in those of the Pythagoreans, that the two criteria will present themselves, misleadingly, as competitors. Thus it is not by abandoning the Aristoxenians' allegiance to perception that Ptolemy can show them to be wrong, but by applying their own criteria in the right way.

We need not spend long on I.11 at this stage. Here Ptolemy offers a refutation of the Aristoxenian thesis that the octave consists of six tones, a proposition that in any case stands or falls with their estimates of the sizes of the fifth and the fourth. (All parties agree that the fifth and the fourth together make an octave.) He chooses to show that the thesis is wrong by a practical demonstration, one that calls for an instrument with eight strings. Much of the chapter is devoted to an account of the way this demonstration is to be set up, and to a description of techniques for ensuring that the instrument itself is accurate. These issues will be considered in Chapter 10.

What underpins the demonstration, of course, is a piece of simple arithmetic, which readily shows that the ratio of an interval spanning six tones, $9^6:8^6$, is not the same as the ratio of the octave, 2:1, but is slightly greater – greater in a ratio close to 65:64, according to Ptolemy (26.1–2), an approximation that is not far out. The arithmetical proof was not new. It is stated in the *Sectio canonis* (Proposition 14, depending on Proposition 9), a work with which Ptolemy was certainly familiar, as we have seen. It is characteristic of Ptolemy that he is not content with this 'rational' argument as it stands, but insists on finding a way of bringing it to the judgement of the ears. Similarly, the *Sect. can.* has its own simple mathematical proof (Proposition 15) that the fourth and the fifth are less than two and a half tones and three and a half tones respectively. The much greater complexity of Ptolemy's discussion in I.10, quite unnecessary from a mathematical point of view, is designed to show that it is not an issue over which the mind and the ear need to dispute.

The only other features of I.11 on which I shall comment here appear in its introductory paragraph. The first is a short argument.

If we instruct the most expert musician to construct six tones in succession, just by themselves, and without the aid of other notes attuned beforehand, so that he cannot refer to some other of the concords, the first note will not make an octave with the seventh. Now if this sort of result is not due to the weakness of perception, the claim that the concord of the octave consists of six tones would be shown to be false; but if it is because perception cannot construct the tones accurately, it will be much less reliable in the construction of ditones, from which he [Aristoxenus] supposes that he can discover that the fourth consists of two and a half tones. (25.5–13)

Ptolemy is trying to impale his opponents on a dilemma. Assuming that the facts about the 'expert musician' are as he states, then if the musician's perception was reliable when he formed each tone in his sequence, the Aristoxenian equation of six tones with the octave will be refuted. If on the other hand perception is not competent to attune tones by themselves, we have further proof of the inadequacy of the Aristoxenian treatment of the fourth, since there the construction involved ditones, which are even harder to assess in isolation. As it stands, the second horn of this dilemma misses its mark, for reasons we have discussed. Nevertheless, if the Aristoxenians attempted to rest their case on the reliability of their method of attuning tones through sequences of concords, then clearly, if six tones so attuned were not perceived as amounting to an octave, either they must withdraw their claim about the size of the octave, or they must admit that their method is defective. In fact, of course, if the 'method of concordance' is used and the relevant fourths and fifths are attuned in their proper ratios, the six tones will inevitably exceed the octave.

Finally, we should notice Ptolemy's remark at 25.13–15, immediately after the passage quoted above. 'The following is nearer the truth: not only does the octave not arise, but neither does any other thing arise through the same magnitude of difference throughout.' The sense of this slightly cryptic utterance is not too hard to unravel. Neither the octave nor 'any other thing', that is, any other harmonically significant interval, can be constructed by a concatenation of sub-intervals that are all the same size. Ptolemy offers here no justification for the thesis, but within the framework of his assumptions it is true. This is not because, for some reason yet unrevealed, no such concatenations could ever be accounted musically acceptable.[11] It is because all three of the intervals within which the primary harmonic structures are contained, the fourth, fifth and octave, have ratios of the form $(n + 1):n$. The ratios of the fourth and the fifth are epimoric. That of the octave does not fit the formal definition of an epimoric. But the proofs given by Archytas and the *Sectio canonis* of the theorem that there is no mean proportional, 'neither one nor more than one', between terms in an epimoric ratio, will in fact apply just as conclusively to it (see pp. 65–6 above). Neither 4:3 nor 3:2 nor 2:1 can be represented as the product of any number of equal ratios of integers.

In that case it is futile to look for ways of breaking down any of these fundamental intervals into parts of equal size. Hence, as will shortly appear, all Aristoxenus' harmonic divisions must be wrong, since all their

[11] In practice, all Ptolemy's formally derived attunements, unlike those of most other theorists, are so contrived as to avoid sequences of more than two equal intervals, even in the case where a diatonic tetrachord lies immediately below the tone disjoining a pair of tetrachords.

quantifications of the intervals of the tetrachord presuppose that the fourth (and the tone, another interval of epimoric ratio) can be divided into equal parts. Ptolemy makes little of the point here, but it is fundamental. No *hupothesis* about harmonic divisions can possibly be correct if it is grounded in the appealingly rational-sounding principle that the 'space' within the fourth, or within the octave, should be distributed in equal segments. Then if the correct harmonic divisions are indeed to be grounded in rational principles, as Ptolemy's manifesto requires, the principle of equal division cannot be among them.

7 Ptolemy on the harmonic divisions of his predecessors

By the end of I.11, Ptolemy has elucidated the status of the homophones and concords in the light of his rational *hupotheseis*, derived their ratios, and described techniques for testing them by the criterion of perception. The structure of concordant intervals by which an attunement is framed (see Figure 4.01 in Chapter 4 above) has thus been transformed into a system of interlocking ratios, set out in Figure 7.01.

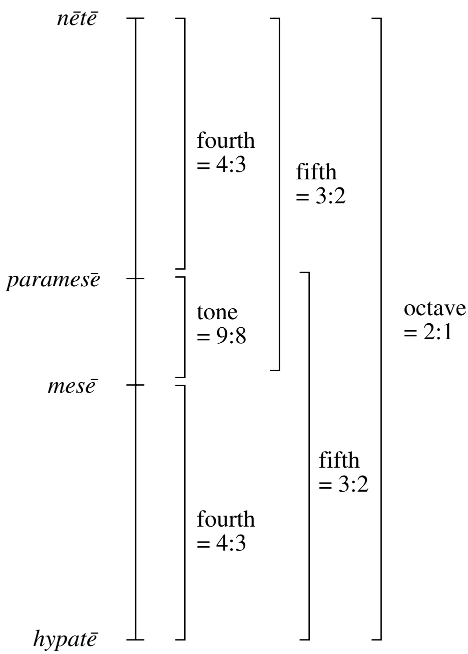


Fig. 7.01

The next task, which occupies the remainder of Book I, is to establish the ways in which a tetrachord can be divided, by the insertion of two further notes between its boundaries. In 'linear' or Aristoxenian terms, the distance between *mesē* and *hypatē mesōn*, for example, must be subdivided into three segments. In the terms proper to mathematical harmonics, the ratio 4:3 must be divided into three lesser ratios, such that when the corresponding intervals are offered to the ear in a particular sequence, it will accept them as constituting a perfectly constructed musical tetrachord. Ptolemy is committed to showing that the perceived aesthetic excellence of an attunement reflects its conformity to intelligible principles of mathematical division and organisation. He will do this by identifying all those forms of division that can be derived systematically from the rational *hupotheseis* he believes appropriate, and then submitting them to the judgement of the ear. He will need to demonstrate that all attunements acceptable to the musical ear can be reproduced in their most aesthetically perfect forms by these procedures.

It is a considerable challenge. The complexities of the task will become clearer if we recapitulate at this point some of the musical background that must be taken into account, and some of the assumptions common to all schools of harmonic theory. Greek musical practice admitted several ways of dividing a tetrachord, to form components of scales or attunements of different types. Each type provided the basis for an aesthetically distinct family of melodies. Three principal kinds or 'genera' of attunement were recognised. They were distinguished from one another by the positions of the inner, 'moveable' notes of their tetrachords, and in particular, according to many authors, by the relations between the higher of these notes and the tetrachord's upper boundary. In an enharmonic tetrachord the interval between the top of the tetrachord and the upper moveable note is large, and the two lower intervals correspondingly small. In diatonic the highest interval is relatively small; and in chromatic its size is intermediate between those of the other two genera. (See Figure 7.02.)

But these are only rough guidelines. The exact sizes of the relevant intervals, or their ratios, remain to be established. Further, though few theorists suggest that there is more than one correct form of enharmonic division,[1] the more thorough of them regularly claim that there are several equally correct variants (called *chroai*, 'shades' or 'colourings' of a genus by Aristoxenus) of the chromatic and the diatonic, each with its own aesthetic peculiarities. Opinions differed about the number of variants in each genus, and about the ways in which their divisions are to be quantified.[2]

[1] But see Aristox. *El. harm.* 22.11–23.22, 48.21–49.21.

[2] In principle, according to Aristoxenus (*El. harm.* 26.8ff), there is no limit to the number of variants there can be. Those described in detail by Aristoxenus himself are only the 'most familiar' (50.19–22).

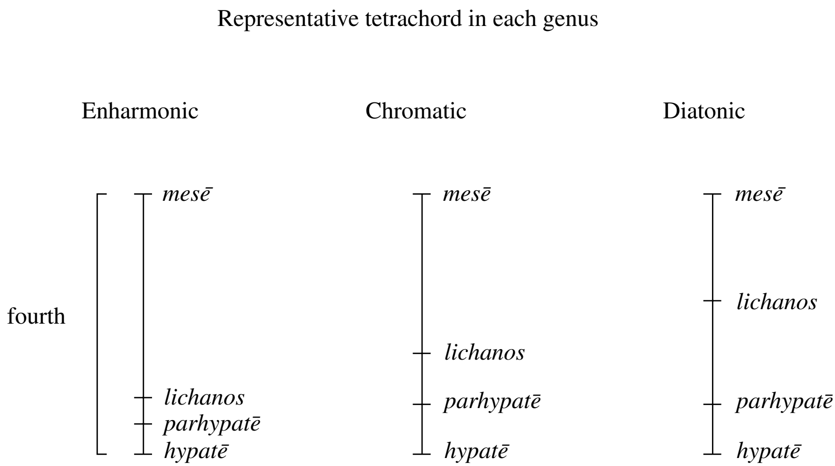


Fig. 7.02

An important preliminary question is raised by the very existence of these distinctions between genera and between variants of the chromatic and diatonic. From the perspective of quantitative analysis, whether expressed in Aristoxenus' manner or in terms of ratios, the genera and their variants differ from one another only in the 'sizes' of the intervals within their tetrachords. On any account of the matter some of these differences are very slight; and the differences between the intervals of a chromatic and a diatonic tetrachord may be no greater than those between the corresponding intervals of tetrachords in two different variants of the same genus. What then justifies the sharp division into three genera, and what distinguishes change of genus from a mere shift between variants of a single genus? Greek writers standardly assume or assert that there are clear differences between the aesthetic characters of the three genera, and they sometimes attempt to explain what they are.[3] But in the context of a scientific harmonics, something less impressionistic is needed, especially if we assume, with Ptolemy, that significant perceptual distinctions must rest upon equally significant mathematical ones.

The theorists generally agree on one such distinction. In enharmonic and chromatic divisions, the two lower intervals of the tetrachord, taken together, are smaller than the highest interval, whereas in diatonic that is

[3] See e.g. Theon Smyrn. 54–6, Aristides Quintilianus 15.21–16.18, 92.19–30 (this passage is probably an interpolation by another writer). Ptolemy himself sketches an aesthetic characterisation at 28.28–29.5.

not so. There, the two lower intervals are jointly either equal to the highest or greater than it. The combination of the two lower intervals in enharmonic and chromatic is known as the *puknon*, meaning roughly something 'compressed' or 'compacted'. It is not hard to guess why the difference between a *puknon* and the rather larger corresponding interval in diatonic should have seemed aesthetically significant. A *puknon*, by this definition, occupies less than half the span of a fourth. It is therefore less, in approximate, Aristoxenian terms, than one and a quarter tones (the ratio of an interval spanning half of a fourth would be a little less than 7:6). Aristoxenus asserts that all *pukna* share a common, perceptible characteristic which larger intervals lack: 'in all *pukna*, though they are of different sizes, the sound of something compressed [*puknos*] is evident to perception (*El. harm.* 48.29–31). Something similar, I think, is true of the way such intervals strike even our modern ears. Intervals of about a tone and a quarter can be grasped as approximating to a sort of minor third, as gravitating towards a 'concord' in the sense familiar to us. Anything smaller approximates to the tone, and is likely to be heard as a 'discord'. Aristoxenus' distinction probably reflects contrasting impressions of much the same sort.

The boundary between the diatonic and the chromatic is crossed, then, when the higher moveable note crosses the point halfway between the top and the bottom of the tetrachord. We can make some musical sense of this, as a significant distinction; and from the mathematical point of view it is at least precise, whether or not any fuller account of its status is available. Our sources are typically less clear, however, about the distinction between the chromatic and the enharmonic. All we are usually told is that in enharmonic the *puknon* is smallest, and that the upper interval in its tetrachord is correspondingly largest.

But if we assume that there is and can be only one form of enharmonic, the distinction between enharmonic and chromatic divisions can be represented in a clearer way. Enharmonic, on that view, would constitute the limit of the contraction of the *puknon* and the expansion of the upper interval; and any division that contains a *puknon* but has not yet reached that limit will be chromatic. This approach presupposes, of course, that there is a quite determinate limit beyond which the *puknon* cannot contract while remaining well formed; and Ptolemy has a way of justifying this view, as we shall see in the next chapter. He also insists that there is only one enharmonic division, so that the style of argument offered above is available for him to use; and a passage in I.15 (34.33–35.1) can indeed be interpreted in precisely this way. No comparably clear distinction was open to Aristoxenus, however, since though he treats as the finest form of enharmonic the one whose *puknon* is at the

limit of contraction (e.g. *El. harm.* 23.3–6), he accepts that divisions with slightly larger *pukna* are also entitled to the name 'enharmonic' (e.g. *El. harm.* 23.17–22, 26.9–11, 49.10–18). If Ptolemy's view rather than Aristoxenus' is accepted, then, the special aesthetic character of enharmonic will be due to its position at the limit of harmonic possibility; and we must expect from him a mathematical explanation for the thesis that this limit is fully determinate.

His own account of the proper procedures for generating harmonic divisions appears in I.15. We shall consider it in Chapter 8. As a preliminary, he devotes the bulk of I.12 and I.13 to descriptions of divisions proposed by his two most distinguished predecessors, Aristoxenus and Archytas. He reserves his criticisms of both until I.14 (though he does not suppress the occasional snort of disapproval in the course of his expositions). I.12 begins with a general introduction to the notions of tetrachordal division and of genus. We can pass over most of its contents here, since the ground has already been covered in my discussion of these matters above, and Ptolemy's account of it is broadly unproblematic.

One feature of his presentation does call for some comment. The passage begins as follows.

Let these points complete our account of the greater differences between notes. We must now turn to the smaller ones that measure the first of the concords, which are found when the fourth is divided into three ratios in the way corresponding to what has already been determined, so that the first homophone, which is one, may be put together from the two first concords, and the first concord from three melodics, up to the number that bounds this proportion (*analogia*). (28.15–21)

It is the last phrase of this passage that is puzzling. Confident interpretation is hampered by the fact that the word *analogia* appears in no comparable context elsewhere in the *Harmonics*, and at least three readings seem possible. First, the word might here be only a variant for *logos*, 'ratio'. In that case the sense is straightforward, though it is oddly expressed. The three melodics must be of such sizes that they jointly complete the span of the perfect fourth; in terms of ratios, when the three melodic ratios are taken in sequence, the first term of the first must stand to the second term of the last in the ratio 4:3. Secondly, the 'number that bounds this proportion' might be the number of different harmonic divisions of the fourth that can be made. Then the remark will mean that the process of 'rational division' must complete this number; it must be exhaustive. Ptolemy certainly intends his to be so. But this sense is not one for which the reader has been prepared, since the proposition that there are more ways than one of dividing the fourth has not yet explicitly been introduced.

The third possibility seems to me to fit the Greek better than either of the others. The word *analogia* has various uses, but the most prominent in mathematical contexts is that of 'proportion', in a sense that specifies a relation between three terms. In this case the 'proportion' in question will be that holding between the three numbers mentioned in this part of the passage, 1, 2, 3, and the 'number that bounds the *analogia*' is simply the number 3. The significance of this number and of the sequence 1, 2, 3 is itself very far from clear, however, and the main interest of the passage is in Ptolemy's curious manoeuvres with these numbers.

He seems to be suggesting that since there is *one* primary homophone, divided into *two* primary concords, the fact that the first concord is to be divided into *three* melodic intervals is natural and intelligible. A couple of sentences later we find two of the numbers highlighted again in an unexpected way. 'The first distinction of genus is into two sorts, corresponding to its being softer or more tense . . . The second distinction is into three, the third being placed somehow between the two mentioned, and this is called "chromatic"' (28.28–29.4). The 'softer' genus is the enharmonic, 'softer' because its moveable notes, and particularly its upper one, are lower in the tetrachord, more 'relaxed' in pitch, while the 'more tense' is the diatonic (29.4–5). We cannot construe the phrases 'first distinction' and 'second distinction' as reflecting the historical hypothesis that chromatic divisions were invented or discovered later than the others. For one thing, most writers adopt a different view.[4] But in any case no such historical assumption would give Ptolemy an adequate reason for considering the process of distinguishing genera, in the abstract, as involving two stages, a division into two followed by a division into three. This treatment seems to have no basis, except in so far as it echoes the processes of division mentioned earlier, first a division of the octave into two, then a division of the fourth into three. It may be that we have here the first hint of the survival, in Ptolemy's dealings with number, of numerological notions in which particular numbers and sequences of numbers are invested with special significance. The prevalence of such ideas in Pythagorean and quasi-Pythagorean sources is well known. In particular, if numbers that play a part in the analysis of one phenomenon could be shown to be repeated in connection with another of a quite different sort, this fact could be used to 'justify' the account given of the latter, since it would provide evidence that this group of numbers constitutes a universal principle of order. It would show that diverse things are united as exemplifications of a single organising pattern.[5] We shall look at this matter more closely in Chapter 8.

[4] See e.g. [Plut.] *De mus.* chs. 11, 20, Aristox. *El. harm.* 19.23–9.

[5] See for instance Aristotle's scathing comments on such ideas, in the closing chapter of his *Metaphysics*; compare Aristides Quintilianus III.6.

Let us now turn to the divisions of the tetrachord attributed by Ptolemy to Aristoxenus and to Archytas. Those of Aristoxenus are faithfully reproduced from his *Elementa Harmonica*. Those of Archytas are recorded in no earlier surviving source, but we have no strong reasons for thinking them spurious, and some of their oddities give positive grounds for assigning them a date no later than the first half of the fourth century BC.[6] Their credentials are further strengthened by the fact that they can be shown to conform to principles of harmonic organisation that were known to Archytas, but which Ptolemy does not mention.[7] The way in which these principles were applied by Archytas is not described in any ancient source; and the process of deriving from them the divisions described by Ptolemy is too technically complex to be plausibly attributed to a Hellenistic forger of 'Pythagorean' documents.[8] Since Ptolemy does not allude to these principles, it is fair to assume that his source (perhaps Didymus) did not indicate any connection between them and the divisions.[9] I shall discuss some aspects of the principles and their application below. For the present, my point is only that there is a presumption in favour of the authenticity of the divisions that Ptolemy attributes to Archytas.

Ptolemy represents Aristoxenus' divisions, as Aristoxenus did himself, in terms of 'linear' distances measured in tones and their multiples and fractions. He does not attempt the impossible task of translating these representations into the language of ratios, or of reformulating them (as he seeks to do in II.13–14) in such a way that we could read off from them the relative lengths of the string of a monochord that would reproduce them in sound. He does indeed offer a second account of each division, one not found in Aristoxenus' surviving writings; but it is no different in principle from the first, and serves only to make comparisons between the different divisions easier. Instead of taking the tone itself as the unit of measurement, it uses the interval of one twenty-fourth of a tone; each interval in each division can then be represented as some whole number of these units.

[6] See especially R. P. Winnington-Ingram (1932).

[7] The principles lie in a classification of mathematical means, recorded in a passage of Archytas quoted by Porphyry at *Comm.* 93.6–17 (= DK 47B2). They are applied to the process of harmonic division by Plato at *Timaeus* 35b-36a.

[8] On the divisions see Barker (1989).

[9] It is nevertheless quite probable that the source quoted Archytas' own abstract statement of the principles in some other context, without any indication of how they were to be applied to a musical subject matter. That is how Porphyry cites them, and his source is likely to have been the same. Ptolemy, along with his authority, may simply have failed to grasp their relevance to the divisions.

Enharmonic	2, ¼, ¼
Soft chromatic	11/6, ⅓, ⅓
Hemiolic chromatic	7/4, ⅜, ⅜
Tonic chromatic	3/2, ½, ½
Soft diatonic	5/4, ¾, ½
Tense diatonic	1, 1, ½

Sizes of intervals are specified in terms of the tone, its fractions and multiples. In each case the highest interval is listed first.

Fig. 7.03

Aristoxenus quantifies six divisions, as in Figure 7.03, one enharmonic, three chromatic and two diatonic. The numbers indicate the sizes of the intervals in each tetrachord, starting from the highest.

Ptolemy makes his first comment on these divisions at the beginning of I.13. It reminds us of the general criticisms of Aristoxenian procedures that were made in I.9. 'From these facts too, therefore, it seems that Aristoxenus gave no thought to ratio, but defined the genera only by what lies between the notes, and not by their differences considered in relation to one another, passing over the causes of the differences as being no causes, as nothings, as mere limits, while attaching the distinctions to things that are bodiless and empty' (30.3–7). The unusual eloquence of this sentence and its carefully contrived rhetorical structure serve to underline the importance Ptolemy attaches to these abstract metaphysical considerations.

He continues with a more specific thrust. 'Hence it is of no concern to him that in almost all cases he is dividing melodics (*emmeleiai*) in half, though those that are epimoric by no means admit such treatment' (30.7–9). Ptolemy is evidently alluding to the fact that in all but one of Aristoxenus' tetrachords the two lowest intervals are equal. That is the only sense in which 'in almost all cases' (all except the soft diatonic) 'he is dividing melodics in half'. He has already argued (16.12–21) that all melodic intervals must have epimoric ratios; hence, by Archytas' theorem, they cannot be halved. It would still not follow that Aristoxenus' halvings are improper, unless it were also shown that the intervals halved are themselves 'melodic' in the appropriate sense, that is, that each can constitute a non-composite scalar step between adjacent notes of an attunement. (Ptolemy cannot mean that every pairing of adjacent inter-

vals must add up to an interval that is in this sense melodic, and must therefore be assigned an epimoric ratio, since this is false of many of the pairs in his own divisions.) But as applied to Aristoxenus' divisions the criticism is cogent, since in every case except the soft chromatic, the interval formed by putting together the two lowest intervals of the tetrachord, which are typically equal, is found elsewhere in the role of a non-composite scalar step. It must therefore be a melodic interval, in Ptolemy's sense, and by his rules its ratio must be epimoric. (Thus the two quarter-tones of enharmonic, for instance, are together the same interval as the half-tone appearing as a simple step in the two diatonics and the tonic chromatic. This half-tone must then be melodic and of epimoric ratio; yet Aristoxenus' enharmonic bisects it.) Hence if we accept that musical intervals must be capable of being expressed as ratios of integers, and that these ratios must conform to the rules Ptolemy has laid down, Aristoxenus' divisions are impossible.

The same principle can be used to undermine the credentials of these divisions even more directly. Each of them without exception presupposes that the fourth itself is divisible into some number of equal parts. If it is agreed that intervals must be expressible as ratios of integers, and that the ratio of the fourth is 4:3, an epimoric, the presupposition will plainly be inconsistent with Archytas' theorem. The two premisses, of course, are ones that Aristoxenus had no inclination to accept. But Ptolemy no doubt considers himself entitled, by this stage, to treat his resistance to them as irrational, resting his case on the arguments of I.9 and I.10.

In I.14 Ptolemy adds three more criticisms of Aristoxenus' divisions. They focus on smaller details. He complains first that Aristoxenus has given the wrong number of variant divisions in the chromatic and the diatonic genera – too many chromatics and too few diatonics. In the case of the chromatic, his distinctions are too fine to be significant: 'the dieses of the soft and of the hemiolic differ by a twenty-fourth part of a tone, which imprints no noticeable variation on the hearing' (32.19–21). The 'dieses' here are the small intervals at the bottom of the tetrachord, forming the *puknon*; one third of a tone, the diesis of soft chromatic, differs by only one twenty-fourth part of a tone from three eighths of a tone, the corresponding interval in the hemiolic variant.

The argument is simple, but its main presupposition is interesting and important. The distinctions made on theoretical grounds in harmonic science must correspond to ones that have the same status at the level of perception. That is, theoretical distinctions have no independent role in harmonics; their function is to elucidate the formal basis of each distinction that perception grasps as aesthetically significant, to translate aesthetic discriminations into mathematical terms. There is no place in

the enterprise for the exhibition of formal differences between modes of organisation which have no counterparts in the perceptible domain. As a criticism of Aristoxenus, however, the argument is weak. His own exposition (*El. harm.* 51.4–7) indicates that he takes the important distinction here not to be the difference between the sizes of the dieses, as such, but the difference between the relations in which each of the relevant *pukna* stand to the tone. The soft chromatic *puknon* differs from the tone by one third of a tone, and the hemiolic *puknon* by a quarter of a tone. Since the former is an interval proper to the chromatic genus, while the latter occurs as an independent interval only in enharmonic, and since the difference between enharmonic and chromatic relations is certainly of aesthetic significance, Aristoxenus could plausibly have retorted that Ptolemy's criticism is misplaced. He could have pointed out, moreover, that Ptolemy is vulnerable to his own argument, since the lowest ratios of two of his divisions, the soft diatonic (21:20) and the tense chromatic (22:21), differ only by one part in 440, which is surely an imperceptible distinction. Ptolemy's response would be roughly similar to the one I have offered Aristoxenus, though more straightforward. The divisions are significantly different, not because of the minute discrepancy in the sizes of these intervals, but because the one division, taken as a whole, is diatonic while the other is chromatic. This distinction is aesthetically important and has been given a clear-cut mathematical interpretation.

The assertion that Aristoxenus' diatonics are too few is not backed up here by detailed reasoning. Ptolemy says merely that 'it is obvious that those which are sung are more, as we shall be able to see from things that will shortly be demonstrated' (32.21–3). It is worth noticing that when Ptolemy comes to his own analysis of diatonic tetrachords in I.15 and I.16, only two varieties emerge quite directly from the application of his preferred method of derivation. A third is added on slightly different theoretical grounds (the tonic diatonic described at 36.20–8), and two more appear in I.16; they too are given theoretical credentials of a sort, but the principal reason for introducing them is simply that they are used in practical music-making. (I shall argue later that Ptolemy's main motive for introducing his tonic diatonic is in fact of the same kind.) Aesthetic and theoretical considerations combine in complex ways in these discussions, and we shall explore them in due course. Here the point is only that Ptolemy has no conclusive 'rational' argument to offer against Aristoxenus' estimate of the number of diatonics. We should also remember, on Aristoxenus' behalf, that he does not intend his list of divisions to be exhaustive, and does not even believe that there could in principle be a complete catalogue of all possible varieties. There is an infinite number of possible positions for each of the moveable notes, within its own limited

range (e.g. *El. harm.* 26.11–19), and any number of these variant positions may be used in practice (e.g. 49.10–14). The divisions he quantifies are only the most 'familiar and noteworthy' (50.19–22). Ptolemy's observation about the diatonics for which detailed analyses are given in the *El. harm.* would have struck Aristoxenus as true but irrelevant.

The remaining criticisms of Aristoxenus' divisions do not call for much comment. Ptolemy objects, once again, to the equality of the intervals forming the *puknon* in his enharmonic and chromatic tetrachords. This time, however, the argument is based on perceptual considerations; the middle interval, he says, 'is always grasped (*katalambanomenou*) as being greater [than the lowest interval]' (32.24–5). (The verb *katalambanein* is regularly and in most cases unambiguously used in the *Harmonics* to refer to perceptual 'grasping'.) Ptolemy offers no theoretical justification for his contention, here or elsewhere, and it is clear that several of his predecessors would have disputed it. Aristoxenus himself enunciates the rule that the lowest interval cannot be larger than the middle one, but insists that it may be either equal to it or smaller (*El. harm.* 52.8–12). Archytas, as we shall see, offered an enharmonic division which ignores even that rule, since its lowest interval is the larger of the two; and the same is true of the chromatic division attributed to Didymus in II.14. If these writers believed that their divisions were even tolerable representations of ones in practical use, practice was evidently too variable (whether between different times or places, or merely between different musicians) for Ptolemy's proposition to hold water as an established datum.

Ptolemy's last thrust against Aristoxenus concerns just one minor detail. The lowest interval of his tense diatonic is identical with that of his tonic chromatic (each is a half-tone). Ptolemy holds that they are different. I now see that I misunderstood his point when writing my translation of the *Harmonics*.[10] There I assumed, carelessly, that he meant that a chromatic interval in this position must be smaller than a diatonic one, and accordingly rendered the phrase *meizonos tou chromatikou sunistamenou* (32.26–7) as 'so making the chromatic too big'. An inspection of Ptolemy's divisions will show, however, that he adopts no absolute rule about the relative sizes of diatonic and chromatic intervals at this place in the tetrachord. Almost all the relevant diatonic intervals are indeed larger than all the corresponding chromatic ones; but since there is one exception, he cannot deploy such a rule against Aristoxenus. And in fact, of course, the phrase means 'whereas the chromatic [interval] is the greater'.

[10] See *GMW2* p. 306 with n.122. I was at least in good company, since the same mistake is made in Düring (1934) p. 197. The same applies to my note about Ptolemy's similar criticism of Didymus in II.13; see *GMW2* pp. 343–4 with n.106.

Then Ptolemy's point must be the very specific one that in the case of *this* form of the chromatic and *this* form of the diatonic, it is (unusually) the chromatic interval that is the larger. He must, in fact, be identifying Aristoxenus' tense diatonic and tonic chromatic with those that constitute the exception in his own divisions, that is, with his tonic diatonic and his tense chromatic. His reasons for this identification are tolerably clear. Aristoxenus and his followers, especially the latter, generally treat their tense diatonic and their tonic chromatic as the commonest and most characteristic division in each of these genera. In many writers no others are considered. Ptolemy, for his part, thinks his own tense chromatic to be the only chromatic in common use; and his tonic diatonic is represented as much the most pervasive of all divisions in the systems of practical music-making (see I.16). Hence he supposes here that the Aristoxenian tonic chromatic and his own tense chromatic must be attempts to analyse the same thing, and similarly for the two diatonics, though the names have been swapped around. The inference is very shaky. Aristoxenians in Ptolemy's era show little sign of having framed their propositions in the light of contemporary practice. They are by and large 'scholastics', content to paraphrase and summarise Aristoxenus' own doctrines, or to quibble about fine nuances of meaning in his terminology. There is nothing unlikely in the supposition that the relation between the lowest intervals in typical forms of chromatic and diatonic tetrachord had altered, in real musical usage, between Tarentum or Athens in the fourth century BC and Ptolemy's Alexandria. There are few signs of historical perspective in Ptolemy's work.

We turn now to the divisions of Archytas. Unlike those of Aristoxenus, they are expressed in terms of ratios, as is proper for a Pythagorean theorist. He offers only three, one for each genus; they are set out in Figure 7.04, with the highest ratio of the tetrachord placed first. The divisions have a number of intriguing and puzzling features which I cannot pursue here.[11] The most inviting target for Ptolemaic criticism is obvious at a glance; the first two ratios of the chromatic are by his standards utterly bizarre. He advertises this oddity even before setting out the divisions. 'Archytas of Taras, of all the Pythagoreans the most dedicated to the study of music, tried to preserve what follows the principles of reason [*logos*] not only in the concords but also in the divisions of the tetrachords, believing that a commensurable relation between the differences is a characteristic of the nature of melodic intervals. But though he sets off from this proposition, at several points he seems to fall hopelessly short of it' (30.9–14).

[11] See the papers cited in nn.6 and 8 above, with *GMW2* pp. 46–52.

Enharmonic	5:4, 36:35, 28:27
Chromatic	32:27, 243:224, 28:27
Diatonic	9:8, 8:7, 28:27

Fig. 7.04

Ptolemy attributes to Archytas, then, the *hupothesis* of which he himself approves, that the difference between the terms of a melodic ratio must be 'commensurable'; that is, it must be a 'simple part' of each term, and hence the ratio must be epimoric (see 16.12–21). If Ptolemy is right, Archytas has plainly compromised this principle in his chromatic division, and the point is made explicitly at 32.1–3. But something must surely be wrong with Ptolemy's interpretation. Archytas cannot simply have been baffled by the arithmetic involved, since there would be no difficulty in constructing a plausibly 'chromatic' tetrachord, very close to that of Archytas in its structure, in which the principle was preserved. Ptolemy's own soft chromatic, whose ratios are 6:5, 15:14, 28:27, differs from this Archytan division only by a slight shift in the pitch assigned to its second-highest note (see the comparative tables set out in II.14). Why then did Archytas not adopt it?

Ptolemy gives part of the explanation himself. 'In the chromatic genus he locates the note second from the highest by reference to that which has the same position in diatonic. For he says that the second note from the highest in chromatic stands to the equivalent note in diatonic in the ratio of 256 to 243' (31.2–6). Ptolemy's expression *phēsi gar*, 'for he says', unambiguously indicates that he took this to be Archytas' own account of the matter, or part of it. Let us assume that he was right. Archytas might have been convinced simply by the evidence of his ears that the second note in chromatic, as attuned by musicians in his milieu, was a little lower than in diatonic. By itself this does not explain why he gives the difference precisely the value he does, but a likely reason is not hard to find.

The ratio between the first and second notes in Archytas' diatonic is 9:8, and the complete span of the fourth is analysable as 9:8 × 9:8 × 256:243, the last ratio being that of the *leimma*. Suppose, then, that the first and second notes of the chromatic stand to one another in the ratio implied in the explanation attributed to Archytas by Ptolemy, that is, in one compounded from the ratios 9:8 and 256:243. In that case the ratio of the second note of the tetrachord to the lowest will be 9:8 (divided in Archytas' system as 243:224 × 28:27). Intervals in the ratios of the 9:8 tone and of the *leimma* are easily attuned through the 'method of concordance' (see

p. 66 above). Very probably Archytas simply observed how musicians located the pitch of the second note in chromatic and its counterpart in diatonic by this method, and drew the appropriate conclusions.

The explanation attributed to Archytas makes it clear that the *leimma* was known to him, and that he treated it as a musically significant relation, despite its curious ratio. We must infer that he was familiar with a division of the fourth as 9:8 × 9:8 × 256:243, which many writers adopt as the correct division for the diatonic. It is implied in the work of one of Archytas' predecessors, Philolaus (fr. 6), and is used by his contemporary, Plato, as the basis of his account of the musical structure of the World Soul (*Timaeus* 34b-36d). Yet Archytas does not himself accept this formulation of the diatonic division. The suggestion made above implies that he found its relations implicated in practical tuning-procedures, in so far as these depend on the 'method of concordance'; and if that is correct, he must have noticed also that musicians did not apply this method in the most obvious and straightforward way in the construction of the diatonic. An application of the method to achieve the broad outlines of the division (giving 9:8 × 9:8 × 256:243) will have been followed by further fine tuning of the third string to a very slightly lower pitch. The case is similar in the enharmonic. The upper ratio of 5:4, a major third, is not quite identical with the true ditone of 9:8 × 9:8, which could be attuned directly by the method. The sort of hypothesis I am offering is supported by a passage in Aristoxenus, where he remarks that contemporary musicians typically 'sweeten' the highest interval in enharmonic by making it just a little less than the two whole tones which he considers correct (*El. harm.* 23.12–22). Probably both he and Archytas noticed that their procedure was first to attune the second string at a ditone below the highest note, by the method of concordance, and then to tighten it slightly, to achieve the 'sweeter' effect they sought.

Apart from the chromatic ratios, the strangest feature of these divisions is that the lowest interval in all three genera is the same. No later theorist accepts such a view. Even if we simply allow, as perhaps we must, that in Tarentine music of this period these intervals were indeed more or less identical, we must ask why Archytas assigned them this ratio in particular. The likeliest hypothesis is that the governing factor is the interval made between the third note of the tetrachord and the note lying a whole tone below its lower limit.[12] The ratio of the interval between this latter note

[12] When the next step below the tetrachord is the tone of disjunction, there will of course always be a note in this position, but this is true only in certain special cases when the tetrachord is in conjunction with the one below it. On traces of the postulation by theorists of a structurally important note in this position see index entries s.v. *hyperhypatē* in *GMW2*.

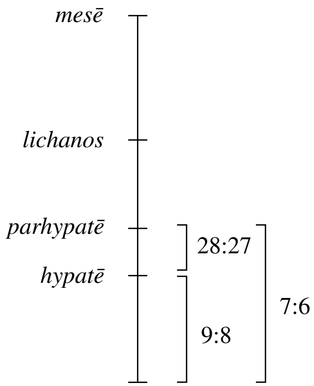


Fig. 7.05

and the third note of the tetrachord, according to Archytas' account, will always be 9:8 × 28:27, which is 7:6 (see Figure 7.05).

From a perceptual point of view, this ratio is that of a variety of the minor third. As an epimoric ratio in fairly small numbers, it is of the sort that Ptolemy would allow as being quite readily identified by the ear, and as 'melodic' in a high degree. If Ptolemy is right, as he seems to be, in supposing that Archytas shared his own view about the special significance of epimoric ratios between small numbers, he will clearly have found this relation mathematically appropriate as a determining element in all attunements.

But we can now begin to see that these epimoric ratios between small whole numbers may have played a rather different part in Archytas' theory from the one they have in Ptolemy's. Though not all his scalar 'steps' have epimoric ratios, the structure of all his divisions is governed by them, and every note can be found, from a given starting point, by moves through just such intervals. A diagram will help to clarify the point (Figure 7.06). The notes named are those of the tetrachord above the disjunction, the tetrachord *diezeugmenōn*, together with *mesē*, the highest note of the tetrachord below.

Archytas' divisions can be constructed, then, by moves through intervals in nothing but epimoric ratios of small numbers, and in this construction every ratio between 3:2 and 9:8 can be deployed. We may add, of course, the ratio of the octave, 2:1, as that of the interval between the outer notes of two tetrachords disjoined by a tone; and in this way we make use of every number between 1 and 9, and no others.

It is a neat result, and points towards another interesting conclusion. I

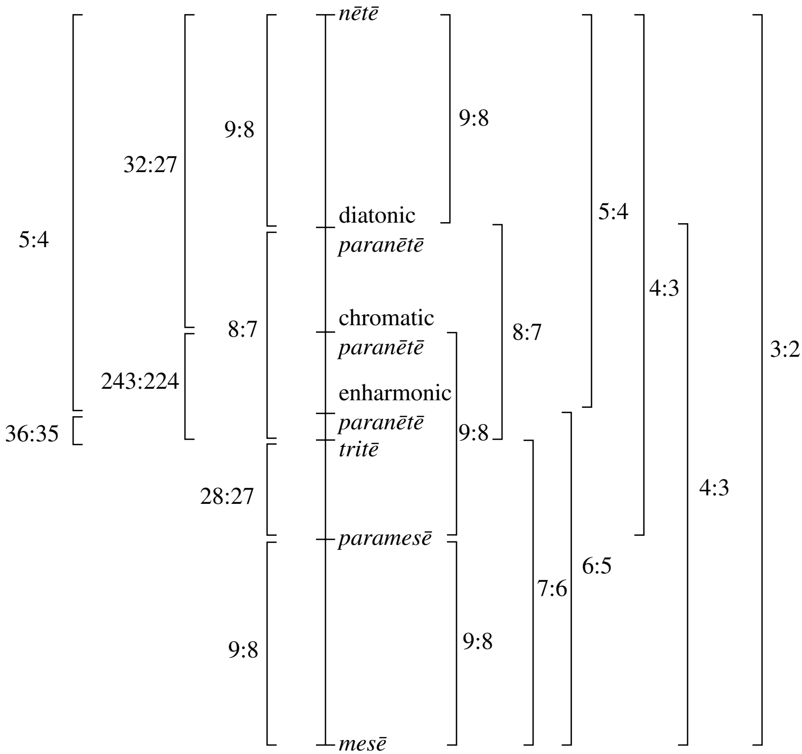


Fig. 7.06

mentioned earlier a principle which Archytas is said to have applied to musical analysis; a short fragment outlining it is quoted by Porphyry (*Comm.* 93.6–17), but without any hint of the way in which it was used. It states that there are three kinds of mathematical 'mean', all of which are 'used in music'. These means are named as 'arithmetic', 'geometric' and 'harmonic'. Briefly, a term M is the arithmetical mean between A and B if A-M = M-B. It is the geometric mean if A:M = M:B. It is the harmonic mean if A-M is the same fraction of A as M-B is of B, or equivalently, if (A-M)/A = (M-B)/B.

Now the geometric mean has no place in the division set out, but it has in the construction of a series of octaves, whose notes will be related as 1:2:4:8, etc. Here each intermediate term is the geometric mean between its neighbours. If we next take two terms in octave ratio (a convenient pair is 12 and 6), and then locate the arithmetic mean between them (9) and the harmonic mean (8), it turns out that these give the division of the

octave into two fourths separated by a 9:8 tone (12:9, 9:8, 8:6, where 12:9 = 8:6 = 4:3, the ratio of a fourth), or into a fourth and a fifth (12:9 = 4:3 and 9:6 = 3:2, the ratio of the fifth; and 12:8 = 3:2, 8:6 = 4:3).

This much use was made of Archytas' principle of 'musical means' by Plato in the *Timaeus* (35b-36b). If we next insert such means, similarly, between terms related in the ratio of the fifth, 3:2, they will fall in such places as to generate the ratios 5:4 and 6:5 between means and extremes. (Thus if we represent 3:2 as 30:20, the means will be 25 and 24.) Placed in the ratio of the fourth, 4:3, they produce the ratios 7:6 and 8:7 (as in the sequence 56, 49, 48, 42). Hence all the ratios used in the construction of Archytas' divisions can be formed by locating the appropriate musical means first in the octave, to create the lesser concords, and then in each of those concords in turn. It appears that Archytas was concerned not only to capture in ratios the real attunements of musical practice, as I suggested above, but also, as Ptolemy says, 'to preserve what follows the principles of reason not only in the concords but in the division of the tetrachords' (30.10–12). On both counts his endeavours parallel Ptolemy's own, and should to that extent have met with his approval.

In considering Ptolemy's criticisms, we should notice first that if I have been right in my reconstruction, he appreciates neither the real function of epimorics in Archytas' scheme, nor the role of the theory of mathematical means, which he does not even mention. He identifies faults of three sorts in the Archytan divisions.

First, he makes explicit his view about the puzzling form of the ratios in the chromatic division. Archytas put his tetrachord together, he says, 'contrary to the premiss (*prothesis*), as we have said' since two of its ratios are not epimoric (32.1–3). The premiss in question is of course the one that Ptolemy himself thinks correct; but he certainly intends to ascribe it to Archytas too, since his 'as we have said' must refer back to 30.10–14, where the attribution is explicit (the word *prothesis* is also used here). This premiss entails that all melodic ratios must be epimoric, a conclusion with which Archytas' chromatic ratios conflict. But I have argued that Archytas' divisions can be so construed as to represent a perfectly consistent application of a principle giving primacy to epimorics. If he did indeed hold that attunements must be built wholly out of epimoric ratios, he must have understood the notion of 'building an attunement' in a non-Ptolemaic way. Ptolemy appears to have missed the point. He would still be entitled to argue, of course, that Archytas had failed to apply the *prothesis* in the right way, but that is not the same as accusing him of inconsistency, as in fact he does.

The second difficulty, according to Ptolemy, is that Archytas' divisions are 'contrary to the plain evidence of perception' in respect of the

chromatic and the enharmonic (32.3–4). He mentions three such errors. First, we grasp (*katalambanomen*) the lowest ratio 'of the familiar chromatic' (*tou synēthous chrōmatikou*) as greater than 28:27. Secondly, Archytas makes the lowest ratio in enharmonic equal to those in the other genera, whereas it actually 'appears' much smaller than them. Finally, he makes this enharmonic ratio larger than the middle ratio of the division, 'though whenever it occurs such a thing is always unmelodic' (32.4–10). Of these complaints the first seems a little specious. It is true that the corresponding ratio of Ptolemy's tense chromatic, which he says is the only chromatic in ordinary use (38.1–6), is greater than 28:27 (he quantifies it as 22:21). But he admits a 'soft chromatic', whose lowest ratio is 28:27, as perfectly proper from the point of view of theory (35.4–6); and in fact the other ratios in this division differ only very slightly from those of Archytas. Yet it does not seem to have occurred to Ptolemy that Archytas' chromatic might be considered as a version of his own soft chromatic. Since he assumes (and I think with good reason) that Archytas had his eye on attunements used in real musical practice, he jumps immediately to the conclusion that what Archytas was trying to analyse was the one chromatic that he, Ptolemy, knew as 'familiar'. He neglects, once again, the possibility that musical conventions had changed over time.

Ptolemy's treatment of the ratio 28:27 as one not recognised by the ear as chromatic, because not in familiar use in that role, should perhaps have made him hesitate also in what he says about Archytas' second 'error'. He claims that the lowest enharmonic interval 'appears' much smaller than its counterparts in the other genera. But the same passage which asserts that the tense chromatic is the only one in use (38.1–6) also tells us that the enharmonic genus is quite unfamiliar to the contemporary ear. Perception will not recognise even the 'soft' version of the chromatic, and hence it can hardly be in a position to decide on the exact form of the division proper to the enharmonic. Certainly Ptolemy has the weight of most earlier writers on his side in claiming that the lowest enharmonic interval is smaller than any ratios in chromatic or diatonic. But he cannot rely on that, since the difficulty for Archytas is explicitly said to arise from the evidence of perception. The problematic status of Ptolemy's criticisms here will be reflected later in questions about the way in which the credentials of some of his own divisions could possibly be assessed (see pp. 145–6 below).

I have already made some comments on the principle behind Ptolemy's rejection of the way in which the middle and lowest intervals of Archytas' enharmonic are related (p. 119 above). He is perhaps on stronger ground here than in his critique of Aristoxenus, however, since the Aristoxenian version of the principle will itself rule out divisions like Archytas', in

which the lowest interval is larger than the middle one. Once again, of course, we can only guess at whether Aristoxenus' view would have been correct, as against that of Archytas, in the period when Archytas wrote. It may be said in defence of Ptolemy's neglect of the possibility of historical changes in musical practice that he was committed to treating the relevant principles as necessarily unchanging, since they are 'rational' and timeless. Hence he could not be expected to allow that what is musically correct becomes different at different times. But while this will certainly be true of principles embedded in 'rational *hupotheseis*' or derived from them, the criteria appealed to here are quite different. At no stage in the *Harmonics* does he suggest that 'errors' of the sort attributed to Archytas under the present heading, that of offences against the evidence of perception, can be shown to be offences also against reason. Some of the rules governing proper attunement are accepted by Ptolemy on the basis of perception alone, and are given no mathematical interpretation or justification. We shall discuss the problems this raises in Chapter 8. Meanwhile if it is perception and nothing else that is authoritative about the issues considered in the present passage, Ptolemy has no good reason for ignoring the possibility that the aesthetic tastes and judgements characteristic of one period may differ from those of another.

Ptolemy's final comment on the Archytan systems is again parallel to one he directs at Aristoxenus. Like Aristoxenus, but still more obviously, Archytas was wrong about the number of variant divisions proper to each genus (32.15–18). From a historical perspective we cannot say much about this thesis. Though distinctions between such forms of attunement as these are likely to have been present in earlier music, we know of no attempt before Archytas to identify, classify and systematise them. It is possible that the notion of 'genus' itself as a classifying concept was an invention of Tarentine musicologists of the fourth century.[13] In that case it would not be surprising if Archytas' pioneering attempts at sorting

[13] Indications that what Aristoxenus calls differences of 'genus' were identified in a manner comparable to his and treated as significant before his own time are very few indeed. It is true that the harmonic divisions found in Philolaus and Plato are in Aristoxenian terms diatonic, while he himself asserts that his (empiricist) predecessors had studied only the enharmonic (*El. harm.* 2.7ff); but with one minor exception (the so-called 'Hibeh musical papyrus', *Pap. Hib.* I.13) the terms 'enharmonic', 'chromatic' and 'diatonic' are used by no pre-Aristoxenian sources at all. (We do not know what names Archytas applied to the systems he quantified.) There is no trace of the concept of genus, under any name or description, in any of the copious musical discussions of Plato, Aristotle or any other fourth-century writer apart from Aristoxenus himself; all their analyses are conducted in terms of the quite different concept of *harmonia*. The fact that when Aristoxenus talks about enharmonic, chromatic and diatonic systems he calls them 'genera', adopting the word *genos* from the repertoire of Aristotelian science, supports the suggestion that the classification was not in common earlier use; if it had been, the heading under which it made its distinctions would surely have acquired a name of its own already.

divisions into distinct 'generic' types were rather too schematic. From the point of view of Ptolemaic theory, however, our earlier difficulties reappear here too. If Ptolemy thought of Archytas as engaged only in the exploration of what is 'rational', his previous introduction of criticisms based solely on what perception accepts will have been inappropriate. If this latter consideration is authoritative, on the other hand, then Ptolemy's own contentions at 38.1–6 will entail that there are really no enharmonic divisions at all, and only one chromatic, though several diatonics will admittedly be allowed.

Even if we can find a tolerably consistent interpretation of Ptolemy's attitude to his own divisions, as I shall argue later, it will not be clear that his criticisms of his predecessors are warranted. Reflecting on the first and second kinds of comment he has made on Archytas' divisions, Ptolemy says this. 'These things, then, seem to set up a slanderous accusation against the rational criterion, since when the division of the *kanōn* is made according to the ratios set out by his proposals, that which is melodic is not preserved. For the majority of those set out above, and of those that have been worked out by virtually everyone else, are not attuned to the characters generally agreed on' (32.10–15). The general theme is by now familiar. Errors made by those who try to proceed rationally, by the application of mathematical criteria, and which are shown up in the conflict of their results with perception, should not be construed as proofs that the search for rational principles is itself misguided (see 15.3–4, and compare 23.19–20). What is becoming progressively more problematic in this connection is the perceptual criterion itself. If perception is competent to judge only proposed representations of attunements in practical use (since in no other case will it have a recognised standard to judge by), it is not competent to pronounce on several of the matters at issue between Archytas and Ptolemy here, nor on some of the divisions derived rationally by Ptolemy in the sequel. If on the other hand Ptolemy means us to believe that perception can reliably assess divisions representing attunements with which it is not familiar in musical practice, and which it does not even enjoy (38.4–5), it is not clear what criterion he thinks it is applying. If it does not enjoy the putatively 'correct' relations, what distinctions is it making in treating them as genuinely melodic, while rejecting others as unmelodic? And if this can be satisfactorily explained, why should Archytas' estimate of the lowest chromatic ratio be rejected merely on the grounds that it is not the one belonging to the 'familiar' form of the chromatic? The problems raised here cannot, I think, be resolved in a wholly adequate way. But before we can assert that with confidence, we need to explore more fully the relations between rational and perceptual criteria in Ptolemy's own constructions.

APPENDIX

It seems appropriate, in this connection, to consider briefly Ptolemy's remarks on the divisions proposed by another theorist, Didymus, whom we have already met in his role as a source of historical material. Ptolemy describes and criticises them in II.13, and the tables in II.14 include both them and those of Eratosthenes, along with Ptolemy's own and those of Archytas and Aristoxenus. On Eratosthenes' divisions he makes no comment; we shall look at them cursorily in another context (p. 254 below). His remarks about Didymus' constructions are for the most part in the same vein as those relating to Archytas and Aristoxenus, and they can conveniently be reviewed here. It is not clear why Ptolemy postpones them until late in Book II. Part of the reason, no doubt, is that Didymus is nowhere represented as one of the great names of harmonic science; he was a minor theorist of the first century AD, not in the same league as the fourth-century pioneers. Hence he does not merit independent treatment at the outset. But in II.13 Ptolemy has just had cause to mention him in a different connection; and this may quite casually have reminded him that he also had divisions of his own to propose. If Didymus was also his source for Eratosthenes, Ptolemy's inclusion of the latter's divisions too, without explanation or comment, may again be little more than an accident. They happened to be set out, perhaps, in the same book by Didymus, and in the part of it that had provoked some of Ptolemy's other reflections in II.13. (This hypothesis is supported by a feature of Eratosthenes' divisions that will be touched on later.)

There is an oddity about Ptolemy's account of the divisions of Didymus. When they are first introduced, it is with the following comment. 'As to the ratios of the division, . . . while he himself posits three genera, diatonic, chromatic and enharmonic, he makes his divisions for only two genera, the chromatic and the diatonic . . .' (68.15–19). Yet an enharmonic division is given under Didymus' name in the tables of II.14, and is described briefly in the text at 71.4–5. At least two sorts of explanation seem possible, assuming that the text has not been interfered with by interpolators. One would be that Ptolemy was just careless; and that after locating Didymus' analysis of the enharmonic, which he had previously overlooked, he forgot to go back and correct what he had said. Another suggestion might be that after having failed to discover such an analysis, Ptolemy simply invented a suitably 'Didyman' one to fill the gap. As we shall see in a moment, this would not have been difficult. But it is easy to find objections to either of these hypotheses. Other possibilities could doubtless be suggested, but I shall leave them to the reader's own ingenuity.

The three divisions presented in II.14 are set out in Figure 7.07, with the ratio of the highest interval in the tetrachord placed first.

Ptolemy's general comments are by now in a well-worn vein. Didymus 'takes no account of the consequences of what is perceived' (68.16), and he breaks various 'rules' about the ordering of ratios and the relations between their sizes (68.20–32). 'The reason for all these things,' he continues, 'was his failure to embark on the imposition of the ratios with sufficient circumspection, having failed to consider in advance the way in which they are used in practice; only this makes it possible for them to be brought into conformity with the impressions of the senses' (68.32–69.1). This sounds fairly vague; the sequel is more precise.

Enharmonic	5:4, 31:30, 32:31
Chromatic	6:5, 25:24, 16:15
Diatonic	9:8, 10:9, 16:15

Fig. 7.07

Whereas his predecessors' construction of the concords was adequate, since they can be brought to the judgement of the senses by divisions of a single string, complete arrangements of melodic intervals cannot be reliably assessed in this way. 'They would be plainly refuted if one were to construct the divisions they propose on the eight strings of equal pitch that we have discussed, these being adequate to display to the hearing the sequence belonging to a melody, so that the genuine and the spurious can be distinguished' (69.5–8).

The point is an interesting one. Didymus' divisions, by Ptolemy's criteria, are seriously wrong, and the fact can be brought out by reference to various rules which they break. If, then, the reason why he and others fell into the traps they did was that they lacked an experimental device on which their divisions could properly be tried out, we are evidently to infer that the rules themselves cannot be extracted straightforwardly from the data of unassisted hearing. Perhaps, like Aristoxenus, we can guess at some of them. But to determine, for instance, which of two adjacent ratios of similar sizes is really the greater, when the attunement is presented in a form acceptable to the hearing, is not something that can confidently be done without the right technical equipment. In the absence of such equipment, then, we cannot formulate reliable rules about the relative sizes of intervals in those positions. We cannot be sure of our judgement in such matters when we merely listen to the notes of such instruments as the lyre; and the monochord is too crude and clumsy a device to produce the notes of a complete division both accurately and quickly enough for the ear to decide on their credentials (see II.12 and the opening of II.13). Not only the testing of *hupotheseis*, then, but also the extraction of plausible rules from perceptual experience, in a sufficiently exact form, demands the construction and use of appropriately elaborate experimental instruments.

Most of Ptolemy's detailed criticisms are repetitions of points made against Aristoxenus and Archytas, and we need not revisit them. One criticism is new, and one expository remark has independent interest. Let us take the latter first. He points out that in both the diatonic and the chromatic divisions the ratio between the highest note of the tetrachord and the third note is 5:4 (68.20–22). At this stage he is still claiming that Didymus presented no enharmonic division; but when that division appears, in II.14, its highest ratio is also 5:4. If this enharmonic is spurious, being Ptolemy's own suggestion to complete the set, the feature he has noticed in Didymus' other divisions provides him with an obvious starting point for his forgery. But this would give him no reason, unless it is merely a desire to discredit Didymus, to divide its *puknon* as 32:31 × 31:30, instead of in the way he thinks correct, as 24:23 × 46:45. The whole 'forgery' hypothesis is pretty lame, in fact; if it were correct, Ptolemy would surely have avoided the indications in the

text that raise suspicion in the first place. It seems much more likely that the division is genuine, and that it is the presence in it of the ratio 5:4 that determines that ratio's role in the others. For a theorist committed to the principle that melodic ratios must be epimoric, as Didymus plainly was, 5:4 is the only plausible candidate for the highest position in enharmonic. His decision to make the two upper ratios in diatonic and in chromatic jointly equal to 5:4, or equivalently, to make the lowest interval in each of those genera equal to the enharmonic *puknon*, is probably due not only to a penchant for neatness of construction. It is likely to represent Didymus' attempt to preserve the relations holding between the most familiar of the Aristoxenian divisions, in which the lowest interval in chromatic and diatonic is a half-tone, and the enharmonic *puknon* is made up of two quarter-tones. We shall return to attempts to convert Aristoxenian divisions into systems of ratios in Chapter 10.

Ptolemy's new criticism is that 'in diatonic he has made the highest ratio greater than the middle one, when the opposite should be the case, as the simple diatonic has it' (68.29–30). The expression 'the simple diatonic' (*to haploun diatonikon*) is a little enigmatic. It seems to mean something like 'the diatonic as it is in its primary and natural form', 'the essential diatonic'. That is, it is a diatonic that has not been modified or nuanced in any special way. Certainly it cannot refer to any and every proper form of the diatonic, since only two of Ptolemy's own five diatonic divisions are consistent with the rule stated here. The key to the puzzle is in Ptolemy's next remark, where he repeats the gist of a criticism made against Aristoxenus, that 'he has made the lowest ratios of the two genera [chromatic and diatonic] equal, when that of the diatonic ought to be smaller' (68.30–32, compare 32.25–7). This makes sense only if the 'correct' chromatic and diatonic that Ptolemy has in mind are his own tense chromatic and tonic diatonic (see pp. 119–20 above). It is virtually certain, then, that the 'simple diatonic' of 68.30 is also this tonic diatonic, which does indeed have the feature to which Ptolemy refers (its division is $9:8 \times 8:7 \times 28:27$). The detail is not insignificant. There are aspects of the way in which that Ptolemaic division is reached which sit uncomfortably with any attribution to it of a peculiarly 'natural' or 'essential' character, and will lead us to raise difficult questions about Ptolemy's application of his own methodological principles. These issues will be opened in the next chapter.

8 Melodic intervals: *hupotheseis*, derivations and adjustments

'Since, then, not even these people have divided the primary genera of the tetrachords in a way that agrees with perception, let us ourselves try, here as well, to preserve what is consistent both with our *hupotheseis* concerning melodic relations and with the appearances, in accordance with those conceptions of the divisions that are primary and natural' (33.1–5). So begins 1.15. From a technical point of view this long chapter is the core of the *Harmonics*, and the analyses that it contains provide the basis for all Ptolemy's later constructions. It will be as well to remark at the outset, however, that they are not his last word on the division of tetrachords. His object is to identify the rational credentials of systems that perception will recognise as perfectly formed. The divisions derived here are perfectly formed from a rational perspective, and in a certain sense from that of perception too; yet it turns out that few of them are acceptable in musical practice precisely as 1.15 describes them. The relation between theoretical perfection and aesthetic acceptability is more complex than has so far emerged. Ptolemy is probably alluding to distinctions of this sort when he describes the conceptions developed here as 'primary and natural'. They constitute in some way both the mathematical and the aesthetic foundations of the systems used in practical music-making, without being quite identical with them.

After the sentence quoted above, Ptolemy proceeds at once to a formal statement of the principles that will govern correct tetrachordal division. They fall into two groups, one grounded in reason, the other in perception. His account of the former group begins as follows. 'To find the positions and orders of the quantities, we adopt as the primary *hupothesis* of reason that all the genera have the following feature in common: that in the tetrachords too, the successive notes always make those epimoric ratios in relation to one another which amount to divisions into two or three that are nearly equal' (33.5–9).

The gist of this *hupothesis* is already familiar from its enunciation at 16.12–21; on pp. 79–87 we examined its general sense, and the ways in which it is linked to Ptolemy's statements in 1.1 about perception and its

data. But there is an obscurity to be unravelled in the closing words of the present passage. When the principle associating melodic intervals with epimoric ratios was first introduced, we were told that 'those which make divisions most nearly into halves must be more melodic, . . . for these, too, are nearer to the equal, just as the half is nearest of all, then the third, and then each of the others in turn' (16.17–21). We might not unreasonably suppose that the 'near-equality' referred to in the present sentence is of the same sort, and that Ptolemy is again alluding to the priority of ratios between smaller whole numbers.

Though at one time I accepted this interpretation,[1] I no longer think it is correct. Earlier in I.7 Ptolemy had used the notion of near-equal division in a different way, to describe the manner in which the fourth and fifth divide the octave, and their ratios divide the ratio 2:1 (15.29–16.6). In I.15 what is to be divided is the fourth, and the ratio 4:3. Ptolemy's procedure will involve two main stages. The first divides the ratio 4:3 into two that are, in a rather extended sense, 'nearly equal'. The second takes one of these sub-ratios, divides it into three that are very nearly equal, and then puts two of them together again to leave a pair of significantly unequal sub-ratios. The completed tetrachord thus contains one of the ratios produced by the first phase of division and two formed by the second. The process has created a division of the fourth into three by beginning from a division into two, and continuing by a procedure that divides a sub-ratio into two after a step involving its division into three. These various 'divisions into two or three' are the ones to which our present sentence refers. The relevant 'near-equality' holds not between the smaller term of a ratio and the difference between its terms, as at 16.17–21, but between the ratios into which a larger ratio is divided by smaller ones, as at 15.29–16.6.

But we must explore more thoroughly the reasons behind Ptolemy's insistence that the divisions must be 'into two or three'. They are not straightforward. We know on other grounds, of course, that the tetrachord must be put together out of three intervals or ratios; that is a simple datum of musical practice. But Ptolemy's remark implies more than that, as the outline above has indicated. In the next few sentences he offers an elaborate and fairly baffling explanation.

By these divisions the differences in the first concords were also found to be bounded, and they go up only to the number 3 there too, since that completes all the intervals. For beginning from the octave homophone and the duple ratio, in which the difference between the extremes is equal to the one that is exceeded, we took for its reduction by equals the hemiolic ratio of the concord of a fifth, in

[1] See *GMW2* p. 306 n.124.

which the difference between the extremes contains a half of that which is exceeded, and the epitritic ratio of the concord of a fourth, in which the difference between the extremes contains a third part of that which is exceeded; and for its augmentation by equals we took the triple ratio of the concord of an octave and a fifth, in which the difference between the extremes makes two of that which is exceeded, antithetically to the half, and the quadruple ratio of the double octave homophone, in which the difference between the extremes makes three of that which is exceeded, antithetically, once again, to the third part. (33.9–22)

These reflections seem reminiscent of a passage we considered earlier, where Ptolemy was introducing the notion of tetrachordal division. The fourth, he said, is to be divided into three ratios, 'so that the first homophone, which is one, may be put together from the first two concords, and the first concord from three melodics' (28.19–20). There too, and in the immediate sequel, we found some puzzling manoeuvres with the numbers 2 and 3. But though the context is similar, the ideas now being brought into play are different. Despite what I said above, the 'divisions into two or three' to which Ptolemy draws attention in 33.9–22 are not divisions of a large interval or ratio into smaller ones. The division of the octave into a fifth and a fourth is indeed mentioned. But it is not the fact that this is a division into two which Ptolemy now emphasises. It is that in the ratio of the fifth, 3:2, the difference between the terms is half the smaller term, while in that of the fourth, 4:3, it is one third of the smaller term. The relevant divisions into two or three equal amounts are those divisions of the smaller term of a ratio which provide a measure of the difference between it and the larger term.

The process of dividing an octave into these sub-intervals is described as its 'reduction by equals'. Let us consider what is involved if we perform this 'reduction' on lengths of string. We begin from the note sounded by a given length, which we shall treat as 12 units long. We now 'augment' the pitch by an octave, which in practice means reducing the length by half, to 6. We are now to 'reduce' this pitch 'by equals' to the original one; and this reduction will involve increases in the lengths of string. The first step is through the interval of a fifth, and the 6-unit length of string must be extended in the ratio 3:2, to give a length of 9 units. This requires us to increase the 6-unit length of string by one half of itself. Hence to acquire the necessary 'measure' by reference to which the operation can be conducted, we need only halve the smaller term, 6. We next 'reduce' the pitch through a further fourth; by increasing the 9-unit length in the ratio 4:3 we reach the original 12-unit length. To provide the necessary measure for this step, we divide the 9-unit length into three, since it is to be extended by one third. Thus our 'reductions' from the pitch of the higher octave have involved equal divisions of lengths, first into two equal seg-

ments, then into three. By similar operations we can 'augment' the pitch from that of the 6-unit length to reach the notes an octave plus a fifth and two octaves above that of the original, 12-unit length. (The relevant lengths will be of 4 units and 3 units respectively.)

Divisions into two or into three, in this sense, have no role in the process whereby the ratios inside the tetrachord are constructed, since in all of them the difference between the terms is bound to be less than one third of the smaller term. There can be no strict parallel between the halves and thirds involved in the measurement of concords and the 'divisions into two or three that are nearly equal' from which the ratios of the intervals within the tetrachord are formed. These, as I explained above, are divisions of the fourth itself, or of some sub-interval within it, into two or three lesser ratios. In that case we must return to the conclusion I sketched in connection with the opening of I.12 (p. 114 above), that Ptolemy is prepared to find significance, and justification for his procedures, in the reappearance of the same groups of simple numbers in different contexts, even where their mathematical functions are not at all the same. A few sentences later, as we shall see, the numbers 2 and 3 reappear yet again, and again in new roles; and their occurrence is emphatically underlined, as if it constituted further confirmation that Ptolemy is proceeding on persuasively rational lines. But from a hard-headed mathematical point of view, the suggestion of an intelligible thread linking all the manifestations of these numbers is the merest hocus-pocus.

The 'primary, rational *hupothesis*' that will govern Ptolemy's procedure, then, is that it must involve near-equal divisions of ratios into two or three epimoric sub-ratios. But we have not done with general principles yet. The text continues as follows.

Secondly, on the basis of agreed perception, we adopt similarly, as common to all the genera, the thesis that the 'following' magnitudes [i.e., the lowest intervals] of the three are smaller than each of the remaining ones; as peculiar to the genera that have *pukna* the thesis that the two magnitudes next to the lowest note are together less than the one next to the highest note; and as peculiar to the *apukna* the thesis that none of the magnitudes is greater than the remaining two together. (33.22–7)

The first of these rules is a development of one stated at 32.7–10 and 32.24–5. There it was only relevant for Ptolemy to insist that the lowest interval in the tetrachord must be smaller than the middle one; now we find that it must be smaller than the highest one too. Despite the rejection of the first part of this thesis, explicitly by Aristoxenus and implicitly by Archytas (see p. 119 above), neither they nor any Greek theorist would dispute the new contention; no one offers a primary division of the tetrachord in which the lowest interval is equal to or greater than the highest.

The second rule given here is also agreed on by other theorists, and amounts, in fact, to a definition, since the *puknon* precisely is a pair of intervals at the bottom of the tetrachord, such that they are jointly smaller than the remainder of the fourth.[2] Part of the third rule follows immediately; the highest interval of a tetrachord lacking a *puknon* (that is, a diatonic tetrachord, here called an *apuknon*) cannot be greater than the other two intervals together. The remaining implication of the third rule is that it is unacceptable also for the middle interval of such a tetrachord to be greater than the sum of the other two (the first rule already requires that the lowest interval must be smaller than either of the others). So far as I know this claim about the middle interval of a diatonic tetrachord is made explicitly by no other writer; but all of them in practice abide by the rule in forming their divisions.

Ptolemy emphatically distinguishes the status of these rules from that of the 'rational *hupothesis*' stated previously. The phrase 'on the basis of agreed perception' marks a sharp contrast with the principles underwritten by reason. It seems that he is proposing to adopt certain rules to govern correct harmonic divisions without offering them any rational justification; and if that is indeed what he is doing, he must apparently have lost touch with the primary goal of his investigations and the basic requirements of his method. His object was to show that what perception accepts as a perfectly formed attunement is so because it is governed by rational principles of organisation, enshrined in the *hupotheseis*. In that case, the perceptible attributes distinguishing well formed attunements from other concatenations of intervals must be exhibited as reflections of formal attributes belonging to systems of ratios; and it must be shown that a system possessing these attributes conforms to the *hupotheseis* of reason. In this way alone can the *hupotheseis* be 'saved'. If it turned out that well attuned systems must also conform to rules of which reason can give no account, the enterprise would apparently, to that extent, have failed. From a rational point of view the constraints imposed by such rules would be merely random (compare 5.19–21).

Now the first of these rules certainly does seem to introduce difficulties of just this sort, though they are not quite the ones they might initially appear to be. Ptolemy might be taken to mean, both here and previously, at 32.8–10, that a sequence of three intervals which spans a fourth and whose lowest interval is greater than the others is invariably heard as unmelodic, and that mere observation will show that no such sequence is ever found in Greek musical practice. But this would be false. Such sequences can readily be found in the systems that Ptolemy himself sets

[2] See p. 112 above, and e.g. Aristox. *El. harm.* 50.15–18.

out and treats as well formed. (By way of an example, the sequence 28:27, 8:8, 8:7, reading from the top down, appears in his tabulation of the Lydian *tonos*, taken in the tonic diatonic genus, at II.15, table 2, column 3.) The point is rather that when such a sequence occurs, it will not present itself to the hearing as a tetrachord of the sort considered in the context of the current discussion, one lying between fixed notes, but will be heard as a sequence located in some other stretch of the system. The rule can be reformulated as a statement about the way in which perception will interpret musical relations of a certain sort. It states that unless the lowest of the three intervals is smaller than the others, the ear will not construe the boundaries of the relevant fourth as fixed notes. Hence it imposes no constraints on the way in which the interval of a fourth, as such, can be divided. But it still does play a crucial role in determining how such fourths can be ordered and disposed in the system. More immediately, the structure of fixed notes has already emerged from Ptolemy's 'rational' division of the octave into a fourth and a fifth; and it is precisely the gaps left between the fixed boundaries of the fourths resulting from this operation that are now to be subdivided. Again, there was complete agreement among the theorists that the differences between genera are to be defined by the disposition of intervals, or ratios, in the space between fixed notes (see for instance Aristox. *El. harm.* 21.31–22.21). There is no question, then, of avoiding reference to this rule in the present context. Yet it remains a principle for which Ptolemy offers no rational interpretation, and whose basis in perception alone he deliberately emphasises. There are serious problems here to which we shall return later (see pp. 145–7 below).

The rule about the *puknon* is not of quite the same sort. It imposes no limitations on the ways in which a tetrachord can properly be formed. There can be no rule to the effect that a *puknon* larger than the remainder of the fourth must be rejected as musically improper on perceptual or other grounds, since such a rule would make no sense. If the structure occupies more than half the span of the fourth it is simply not a *puknon*, and will not be perceived as one (see Aristox. *El. harm.* 48.20–31). It will of course follow that if enharmonic and chromatic divisions are distinguished from others as being those that contain *pukna* (29.5–9), then if any division in which the two lowest intervals together are not smaller than the highest is offered as a version of the chromatic, for example, it will be dismissed as non-chromatic on perceptual grounds. But it will not for that reason be unmelodic, since it might still be an acceptable diatonic division. This rule (or definition) does nothing to determine what will constitute a division of the tetrachord that is melodically correct.

But a difficulty remains. The distinction between systems that contain *pukna* and those that do not was apparently felt as aesthetically very significant. Ptolemy's general stance requires that such perceptible distinctions should be exhibited as reflections of formal ones that have comparable rational or mathematical status. While the quantitative distinction between *pukna* and *apukna* can be given a mathematical description that is perfectly clear, Ptolemy has done nothing to show how the line it draws could be conceived as mathematically significant. Its status has not been shown to be derivable from rational *hupotheseis*; and Ptolemy's introductory phrase has apparently indicated that its basis is in perception alone.

The difficulty can be softened but not eliminated. The fact that the line bisecting the tetrachord marks a boundary between aesthetically distinct kinds of division cannot be made to follow from the *hupotheseis* Ptolemy has enunciated. But in a looser sense it fits with them. We have seen several times that the notion of 'equal division', variously interpreted, has a leading role in Ptolemy's conception of what is harmonically rational. Hence a formal description of the difference between *pukna* and *apukna* would draw on the same pool of ideas as that to which the *hupotheseis* belong, and the same terminology as that in which they are expressed. It would not be alien to the mode of 'rationality' that the *hupotheseis* exhibit. Though Ptolemy says nothing here to encourage this interpretation, there are suggestions of it at the opening of I.16. We shall return to the matter there, and to the general issues raised by Ptolemy's use of considerations that 'accord' with his *hupotheseis* but do not follow directly from them.

Meanwhile, let us move on to consider the ways in which Ptolemy applies his principles in the derivation of the divisions themselves. Some of the details of 33.27–37.4 can be passed over or abbreviated here.

We saw earlier that the first phase of the process involves the division of the ratio of the fourth, 4:3, into two lesser ratios, which must themselves be epimoric (33.27–9). This requirement seems at first sight to go beyond any for which Ptolemy has explicitly argued. Melodic intervals, those lying between adjacent notes in an attunement, must of course be epimoric (16.12–21); but only one of the ratios in each pair constructed at this stage will be that of a single melodic step in any given division. The other will be subdivided in the sequel. But in fact Ptolemy is still within his own guidelines. In any one division of the tetrachord, only one of the two ratios in each pair will remain undivided. But it turns out that Ptolemy will insist on using the other, too, as an undivided step in a different division. Hence both must be melodic, and both must therefore be epimoric.

The division of the ratio 4:3 into two epimorics 'occurs only three times' (33.29–30); and Ptolemy is plainly finding food for thought, once again, in what appears to be a quite casual association of the numbers 2 and 3. The thesis is true, however. The ratio 4:3 can be resolved as 5:4 × 16:15, as 6:5 × 10:9, or as 7:6 × 8:7, but into no other epimoric pairs. (The three ratios placed first in these expressions are the next three in order after 4:3 itself, a fact to which Ptolemy also seems to attribute some significance (33.30–34.4).) Since these are the only pairs available, they also count, in a slightly backhanded way, as the three that divide the fourth most nearly into two equal epimoric ratios (see 30.8–9), even though only the last division comes at all close to halving it. Similarly, the ratios 3:2 and 4:3 were treated as the epimorics that most nearly halve the ratio 2:1, on the grounds that there are no other such ratios by which it can be divided into two parts at all (15.30–16.1).

To complete any division, one or other of the ratios in a pair must itself be divided into two. In the cases of the enharmonic and the chromatic, which contain *pukna*, it must be the smaller ratio that is divided, so that the two lower intervals of the tetrachord may be jointly smaller than the highest one (34.5–10). In diatonic divisions, which exclude the *puknon*, the ratio to be divided must be the larger of the two. In dividing the relevant ratio, Ptolemy avoids the procedure that would seem most straightforward. If we take, for instance, the ratio 16:15 and double its terms, giving 32:30, we can find a pair of epimoric ratios that together fill out the ratio 16:15 by inserting the intervening term 31. The pairing will be 32:31 × 31:30. (These are the ratios of the enharmonic *puknon* attributed to Didymus in the tables of II.14. Very probably they were derived by this method.) Ptolemy triples the terms of the original ratio instead of doubling them, giving in this case 48:45. We can then insert terms that divide this ratio into three, as 48:47 × 47:46 × 46:45. By compounding the first two of these ratios, giving 48:46, we arrive at a division of the original ratio into two epimorics, as 48:46 (= 24:23) × 46:45.

Why Ptolemy adopts this strategy is unclear. His own explanation is brief and enigmatic. We divide the ratio into three sections (before reducing them to two) 'because from here the three ratios of the tetrachord are now produced' (34.12–13). This cannot mean that the 'tripling' procedure parallels that by which the three initial pairings of ratios were generated to divide the fourth, since tripling the terms of the ratio 4:3 will give only one of these pairings (6:5 × 10:9). Another can be found by doubling them (7:6 × 8:7). The third, 5:4 × 16:15, can be found by this method only if the terms are multiplied by 4. We seem driven to interpret Ptolemy's remark as meaning only that an operation with the number 3 will be appropriate here, since it will complete the division of the

tetrachord into three ratios. In that case we have yet another instance of Ptolemy's indiscriminate appetite for repetitions of the number 3, no matter how loose their mathematical connections. But we shall come back to the matter shortly.

The procedure is now rigorously applied. When the smaller ratio in each initial pairing is divided, so as to produce a *puknon*, the first pair produces the division 5:4 × 24:23 × 46:45, the second 6:5 × 15:14 × 28:27, and the third 7:6 × 12:11 × 22:21. (In accordance with the first of the rules drawn from perception, at 33.22–4, the smallest ratio always takes the lowest position.) When Ptolemy turns to the divisions that lack *pukna*, and in which it is therefore the larger ratio in each pair that is subdivided, the first of them, 5:4 × 16:15, throws up a difficulty. If 16:15 is left undivided and assigned to the highest interval in the tetrachord, the tripling procedure applied to the ratio 5:4 will yield its subdivision as 7:6 × 15:14. But neither of these ratios can take the lowest position in the tetrachord, since each is greater than the highest. Ptolemy concludes that the ratio 16:15 cannot be used in the highest position in the tetrachord (35.17–36.6). The other two initial pairings can be dealt with straightforwardly by the established method. The first yields the division 8:7 × 10:9 × 21:20, and the second 10:9 × 9:8 × 16:15.

Let us pause here to take stock. Ptolemy's procedure in making these divisions is certainly systematic, but there are questions to be asked about his grounds for thinking it uniquely appropriate. We may reasonably wonder why its first step, the division of 4:3 into two epimoric sub-ratios, should be thought necessary at all. It would be perfectly possible to move directly to a division of the fourth into three. But there are several reasons for avoiding this strategy, quite apart from Ptolemy's apparent obsession with the numbers 2 and 3. One is perhaps theoretically disreputable; there will simply be too many such forms of division. Criteria will have to be introduced at some stage to eliminate the excess, and Ptolemy's first step goes some way in this direction. There are also genuine musical considerations to support the manoeuvre. Many theorists, Aristoxenus among them, show a tendency to regard the interval comprising the *puknon* as a significant musical unit in its own right.[3] Its division into two parts was treated as relatively unimportant, and some theorists, in certain contexts, ignore it altogether.[4] The special character of each generic division was usually held to be determined by the position of the upper moveable note, while the lower one introduced only minor variations.[5] It was

[3] For instance in the long sequence of 'theorems' referring to the *puknon* at *El. harm.* 62.34ff.

[4] See e.g. Thrasyllus' account of the enharmonic, recorded at Theon Smyrn. 93.1ff.

[5] E.g. Aristox. *El. harm.* 22.22ff., 50.15ff.

sometimes asserted, probably correctly, that early musical systems placed only one note between the boundaries of the fourth, and that the subdivision of the lower interval was a later development; and the influence of this ancient practice apparently continued to be felt long after the four-note tetrachord had become the norm.[6] Hence in ensuring that the ratio of the interval comprising the two lowest intervals of the tetrachord together was itself epimoric, and thus of suitable melodic status, Ptolemy's first step reflects real musical intuitions.

The tripling procedure involved at the second step is another matter. We have seen that its theoretical credentials are shaky, and that Ptolemy himself underpins it with no more than a brief and unconvincing aside. This is strange, since it is essential for him to show that only this procedure is legitimately rational. The issue matters, since the application of other methods at this stage will produce several other divisions into epimorics to compete with Ptolemy's. I pointed out above that different results will be obtained if we double the terms of the ratio to be divided, instead of tripling them. Thus the smaller ratio in the first of Ptolemy's divisions, 16:15, which he divides as 24:23 × 46:45, can also be divided as 32:31 × 31:30; and by a route that multiplies the terms by 4, we can also arrive at 21:20 × 64:63. The same operations can of course be performed on the other ratios that are to be divided. Unless there are good reasons for thinking Ptolemy's tripling procedure uniquely well qualified for the task, it will not have been shown that these alternative divisions are less rationally acceptable than his. The one reason at which he gestures seems feeble.

There is, however, one other reason we might offer him; and the point on which it turns falls squarely within the range of considerations which Ptolemy thinks significant. It is best shown by an example. Let us take Ptolemy's first division, the enharmonic of 5:4 × 24:23 × 46:45. If we now consider these ratios as expressions of relations between pitches, not relations between lengths of a string, the larger number in each ratio represents the higher pitch. Starting from the lowest, then, the second note is assigned a number which is 46 of the units of which the first is 45. The third is 48 of the same units, and the highest is 60. Contrast now the case of the most obvious rival division, in which the residue below the interval whose ratio is 5:4 is divided as 32:31 × 31:30. At first sight something similar will be true here, and will use smaller and hence more appealing terms. One term will be made up of 30 units, and the others of 31, 32 and 40 units of the same size.

6 See especially [Plut.] *De mus.* 1134f-1135b. For an example of the consistent omission of the second internal note of the tetrachord, apparently to create a solemn, archaising effect, see the first section of the paian of Athenaios, composed in 127 BC (printed as no. 19 in Pöhlmann (1970), West (1992) pp. 288–93).

But this is misleading. The ratio 31:30 is greater than 32:31, and by Ptolemy's rule cannot take the lowest place in the tetrachord. The sequence, from the top down, will have to be 5:4 × 31:30 × 32:31. Then the second note from the bottom is 32 of the units of which the first is 31, but the third and fourth notes cannot be expressed as integral multiples of the same unit. In order to capture all the terms in this way we shall have to say, in fact, that each is so many units of which the first is 1860 (the lowest common denominator of the three ratios). It will be clear that the large numbers involved here are not an irrelevance, if we recall Ptolemy's representation of musically acceptable intervals as ones in which the business of comparing the terms is relatively simple. In either version of the system the relations between adjacent terms will of course be simple in the appropriate way. But if we try to make musical sense of the division as a whole, we need to make reference to a unit in relation to which all the terms can be measured, and the larger this unit is in relation to the terms compared, the simpler the comparison will be. By this criterion Ptolemy's division is vastly superior.

By now Ptolemy has assigned names to his first three divisions. He prefaces his classification with a short explanation. 'Now since of all the genera the enharmonic is softest, there is as it were a road from it towards the more tense, by a process of increase through first the softer chromatic, then the tenser, towards the succeeding genera that are *apukna* and diatonic. In general those appear softer that have the larger leading ratio, and those appear tenser that have the smaller one' (34.33–35.3). I alluded to these remarks earlier (pp. 112–13). If the description of the enharmonic as the 'softest' (*malakōtaton*) is meant to imply not only that divisions in other genera always have a smaller upper interval, but also that no division in which this interval's ratio falls short of the maximum can count as enharmonic, it will serve to explain the fact that Ptolemy counts only one of his divisions as enharmonic. (It will of course do nothing by itself to show that a division whose upper ratio is also 5:4, but whose *puknon* is distributed differently from Ptolemy's, is not equally permissible and equally enharmonic.) Granted that one of the divisions he offers is enharmonic, and that no more than one can be, the other two in the first group must be chromatic, since all three contain *pukna*. Hence the division into 5:4 × 24:23 × 46:45 is assigned to the enharmonic, 6:5 × 15:14 × 28:27 to the soft chromatic, and 7:6 × 12:11 × 22:21 to the tense chromatic. (The names of subsidiary forms of chromatic and of diatonic vary in our sources, but the nomenclature is not important here.)

Before naming the divisions in his second group, Ptolemy makes a new and surprising move. Up to this point the divisions have been derived from first principles by the consistent application of a systematic method. The method has run its course, and cannot be used to generate any

further divisions. Ptolemy nevertheless wishes to introduce another. His account of it is as follows.

But prior to all these ratios, the ratio 9:8 was found in its own right to contain the tone arising from the difference between the first two concords; and this, according to what is both rational and necessary, ought also to occupy the leading position, those closest to it being conjoined with it, since none of the epimoric intervals fills out with it the epitritic ratio [4:3]. The ratio 10:9 has already been conjoined with it in the division set out above, but the ratio 8:7 has not yet. Hence we shall conjoin this with it, in the middle position, and allocate the remainder making up the epitritic ratio, which is the ratio 28:27, to the "following" [i.e. the lowest] position. (36.21–8)

The procedure used to derive this division is obviously anomalous, by the standards of what has gone before. The ratio 9:8 does not emerge from an initial division of the fourth into two epimorics, since as Ptolemy says, there is no epimoric ratio n:m such that $9{:}8 \times n{:}m = 4{:}3$. The subdivision of the remaining interval cannot then proceed in the usual way, and Ptolemy's choice of considerations to determine it involves two assumptions at least one of which is new. The first, that the ratios closest to 9:8 should be conjoined with it, might be taken in either of two senses. It might mean that in looking for a ratio to place immediately below 9:8 in the division, we should specifically seek one as close as possible in value to 9:8, presumably so that the two successive intervals and ratios may be 'near equals'. That would be a novel requirement in this context. Alternatively, it might mean no more than that of the two ratios chosen to complete the tetrachord, whatever they are, the one nearer in value to 9:8 should be placed next to it. This would, in effect, be only a repetition of the familiar principle that the smallest ratio in the tetrachord must lie at the bottom, and as such would be unobjectionable; but Ptolemy's words are most naturally taken, I think, in the former sense.

Secondly, we are told that of the two possible associates for the ratio 9:8 (that is, the two epimorics that are closest to it in value), the one to be chosen is 8:7, since the other, 10:9 has already been used. (As far as I can see, there is in fact no way of dividing the remainder of the fourth, whose ratio is 32:27, into two epimorics, without including either 10:9 or 8:7 as one of them.) But that involves a new principle, since nothing has been said to rule out the possibility that the same set of ratios may be used twice, in different orders. The division in which 9:8 and 10:9 already appear has 10:9 at the top; and there is nothing obviously improper about one in which the positions of the two highest ratios are exchanged, giving $9{:}8 \times 10{:}9 \times 16{:}15$. Such a division is perfectly consistent with Ptolemy's previous *hupotheseis*, and it is indeed the diatonic division proposed by Didymus, and recorded in II.13–14.

The most striking anomaly, however, lies in the reason given for adopting a division of any sort in which 9:8 stands at the top. The ratio, as Ptolemy says, has a privileged position in harmonic analysis, since it is that of the tone, the difference between the fifth and the fourth. But he does not explain why this fact should justify its adoption as the highest ratio of a division. He merely asserts that the move is 'rational and necessary'. In the absence of any analysis of this 'necessity', this looks like nothing but plausible rhetoric. It is perhaps curious that Ptolemy makes no appeal to one consideration that might well be felt relevant, if rather weak – an appeal, that is, to a requirement of completeness. The divisions set out so far have used as their highest intervals every epimoric ratio from 5:4 to 10:9, with one exception; 9:8 is missing. A sense of neatness and order would indeed suggest that the gap should be filled; yet Ptolemy shows no sign of resting his argument on foundations of that sort.

Two further points about this division are worth noting. First, it is identical with the diatonic attributed by Ptolemy to Archytas in I.13. Secondly, it will be as well to remark at this stage that this newcomer to Ptolemy's set of divisions is not just a casual appendage; in his account of the attunements of practical music-making it turns out to be the most important division of all. We shall review these matters shortly. But this fact makes it all the more remarkable that the division has not emerged by orderly derivation from the rational *hupotheseis*, and that such rational credentials as it has have not been more fully explored.

Three divisions which lack the *puknon* and are therefore diatonic have been set out, and they are now named. The division into 8:7 × 10:9 × 21:20 is that of the soft diatonic; the anomalously derived division into 9:8 × 8:7 × 28:27 is that of the tonic diatonic (because of the tone constituting its highest interval); and the third is the tense diatonic of 10:9 × 9:8 × 16:15.

I.15 ends with a resounding statement of Ptolemy's faith in the consonance of reason and perception, and a thrust, in passing, at his rivals.

The fact that the divisions of the genera set out above do not contain only what is rational but also what is concordant with the senses can be grasped, once again, from the eight-stringed *kanōn* that spans an octave, once the notes are made accurate, as we have said, in respect of the evenness of the strings and their equality of pitch. For when the bridges set under [the strings] are aligned with the divisions marked on the measuring rods placed beside them – the divisions that correspond to the ratios in each genus – the octave will be so tuned that the most musical of men would not alter it any more, even a little. We would be astonished at the nature of the ordering of attunement, if on the one hand the reasoning that deals with it moulded, as it were, and shaped the differences that preserve melody, and if hearing followed the lead of reason to the greatest degree possible (being thus arranged alongside the ordering arising from reason, and recognising the appropriateness of each of its propositions), while on the other hand the outstand-

ing experts in the subject condemned it, though they are unable, by themselves, to initiate an investigation of the rational divisions, and neither do they think fit to try to discover those that are displayed by perception. (37.5–20)

The remarks about the adjustment and manipulation of the instrument called the *kanōn* will be discussed later, in Chapter 10. Ptolemy's allusions to 'the most musical of men', and 'outstanding experts in the subject', seem to be aimed at contemporaries rather than predecessors, at least if the rhetoric of the passage is to be trusted. They have the air of a direct challenge to people who will read Ptolemy's work and could undertake the experiments he requires. No such theorists, however, are named in the *Harmonics*, nor are any doctrines mentioned that cannot be traced back to sources in the previous century, at the latest. We do not know whether Ptolemy had any particular competitors in mind, or who they were if he did. But there seems to have been no shortage of Aristoxenians in the second century; and we have substantial works from the pens of two authors of a more or less Platonist or Pythagorean persuasion, Nicomachus of Gerasa and Theon of Smyrna, both of whom belong roughly to Ptolemy's period. Ptolemy would undoubtedly have found much to complain of in their writings.

From a theoretical point of view, this passage by itself tells us nothing unexpected. But it takes on a new importance when it is juxtaposed with the immediate sequel, at the beginning of I.16.

Now of the genera that have been set out, we would find all the diatonic ones familiar to our ears, but not in the same way the enharmonic or the soft one of the chromatics, because people do not much enjoy those of the characters that are exceedingly slackened, and it is enough for them in the movement towards the soft to get as far as the tense chromatic. (38.2–6)

The main question raised by these remarks is obvious. If the enharmonic and the soft chromatic are unfamiliar to the ears and are not enjoyed when they are heard, how can the credentials of the divisions designed to represent them be perceptually assessed? The perceptible characteristic that marks off well formed from ill formed divisions is their beauty. It is true that Ptolemy later distinguishes beauty from pleasantness, claiming that sight and hearing 'are the only senses that assess their objects not only by the standard of pleasure but also, much more importantly, by that of beauty' (93.13–14). But this is not to say that a thing's beauty can be appreciated by someone incapable of enjoying it. We should notice that Ptolemy's word for 'pleasure' in the later passage, *hēdonē*, which he uses elsewhere with plainly negative intent,[7] is unrelated to the one used for 'enjoy' at 38.4, *chairousi*. *Chairein* appears nowhere

[7] See 97.1, 100.1, and compare *On the Criterion* 21.19.

else in the *Harmonics*, but in standard Greek literary and philosophical usage has no necessary connection with the brute or bodily pleasures, and carries no disapproving connotations.[8] In many reflective writings *hēdesthai* is morally suspect. *Chairein* is not; and it would be very unusual to detach the capacity to perceive beauty of a certain sort from the capacity to enjoy it, in the sense that *chairein* indicates.[9] The difficulty might be alleviated a little if we took Ptolemy's remark about enjoyment to refer only to people of a commonplace sort, 'most people', while allowing that an appreciation of these divisions is possible for experts. There are writers who say things of this sort.[10] But that can hardly be what is intended here. The initial subject of the sentence is not 'they', but 'we'. Further, the word 'people' is itself suspect in my translation. In the Greek there is no noun; and were it not for the gender of the word translated 'for them', *autois*, in the phrase 'it is enough for them', the natural subject for 'do not much enjoy' would be 'our ears' (lit. 'hearings'). One important group of manuscripts in fact has *autais* here, the variant that would be needed for this interpretation; and it may well be correct. In any case the text provides no scope for an escape route through the gap between ordinary folk and persons of refined and educated taste. At the very least, Ptolemy owes his readers an explanation of how the ratios of his enharmonic and soft chromatic can be recognised as 'concordant with the senses' (37.6) by ears that cannot enjoy them. A remark in a similar context later in the *Harmonics* suggests a certain embarrassment about this issue. Ptolemy says that he has adopted a particular strategy 'in order to disguise the fact that even we have gone beyond the limits of what we ought to do, since we have already busied ourselves too much with the divisions of unfamiliar genera' (74.13–15).

Ptolemy now explains why the tense chromatic is found to be aesthetically satisfactory.

For the *puknon*, by which in a way the nature of the soft is distinguished from that of the tense, finds its limit in this genus, beginning from here in the progression towards the softer, and ceasing here again in that towards the more tense. Again, in the segmentation of the whole tetrachord into two ratios, it is defined by the ratios that are nearest to equality and are consecutive, that is, by the ratios 7:6 and 8:7, which divide in half the whole difference between the extremes. For the reasons given, then, this genus seems most agreeable to our ears. (38.6–13)

What is to be accounted for here is a *phainomenon*, a datum of aesthetic experience, the fact that an attunement constructed according to this

[8] In Ptolemy's own writings see for instance *Tetrabiblos* 157.16, where the context is positively laudatory.

[9] See MacLachlan (1993).

[10] E.g. Aristox. *El. harm.* 19.17–29, 22.24–23.22, and the quotation from his work at Athenaeus, *Deipn.* 632a–b; Adrastus quoted at Theon Smyrn. 55–6.

form of the chromatic is perceived as 'most agreeable' (*prosphorōtaton*), while attunements more 'relaxed' or 'softer' than it are not. The two explanations are apparently independent. In neither of them, however, can the facts that are called on be regarded as data of experience of the same sort as the fact to be explained. That the *puknon* finds its limit here, and that the ratios by which the fourth is initially divided are 7:6 and 8:7, are truths revealed by the use of experimental instruments and by mathematical analysis. Hence in Ptolemy's usage they constitute explanations of a 'rational' kind, indicating formal features of the division that are accessible through the use of reason. We are to understand that it is these formal features that are reflected in the vaguely conceived perceptible attribute of 'agreeableness'. Thus the explanations conform neatly to the general thesis that aesthetically significant attributes of musical systems are to be accounted for as the perceptible aspects of intelligible modes of formal structure. To return for a moment to a difficulty we reviewed earlier (pp. 137–8), these are the explanations that give colour to my suggestion about the distinction between *pukna* and *apukna*. Ptolemy is apparently prepared to ground the significance of the formal aspect of this distinction in its connection with equal division. To that extent, then, though the distinction was indeed made 'on the basis of agreed perception' (33.22), its status can be made rationally intelligible.

The thesis that correlations must be made between perceptible and formal attributes and distinctions is of course a guiding principle throughout Ptolemy's investigations. What we are seeing now, however, is that his notion of a 'rational' explanation or analysis is becoming progressively more flexible. We were initially given to understand that a fact of perception would count as rationally explained only if its formal counterpart were shown to be a consequence of the application of high-level intelligible principles, 'rational *hupotheseis*'. In I.15 Ptolemy was prepared to introduce, in justifying the status of his tonic diatonic, 'rational' considerations which have no basis in the *hupotheseis* from which correct divisions were to be derived. In the present passage, neither of the explanations offered for the aesthetic charm of the tense chromatic rests directly on those *hupotheseis*, though both are intuitively appealing, and the second is a variation on a theme that by now is very familiar.

What seems to have happened is that Ptolemy has begun to interpolate into his original strategy some exercises of another, closely related to the first, but noticeably less ambitious. Not all the formal correlates of aesthetic impressions will be shown to owe their status to their derivability from primary *hupotheseis* of an all-embracing sort. Ptolemy is sometimes content merely to identify these formal counterparts, to represent them in language that hints at a relationship with higher *hupotheseis* but falls short

of demonstrating derivability, and to indicate why it is plausible or reasonable to think of these particular perceptible attributes as reflections of just these formal properties. This last aspect of the project is clearly brought out in the continuation of the present passage.

The characteristics of the tense chromatic, Ptolemy says, 'also suggest to us another genus, when we set out from the *emmeleia* [the property of being *emmelēs*, 'melodic'] that is constituted in accordance with equalities, and investigate the question whether there is any agreeable (*prosphoros*) ordering of the tetrachord when it is initially divided into the three nearly equal ratios, again in equal excesses' (38.13–17). The relevant ratios are found by a procedure analogous to that used at the second stage of the regular divisions, by tripling the terms of the ratio to be divided, here 4:3, and inserting the intermediate terms that will make epimoric ratios with both extremes. The ratios produced are 10:9, 11:10 and 12:11 (see 38.17–21).

This division is plainly not derivable in the same way as are the theoretically perfect divisions of I.15. The idea lying behind it is that since the fact that the tense chromatic is very agreeable (*prosphorōtaton*) has been accounted for by the near-equality of the two ratios that most significantly divide its tetrachord, it is to be expected that a similarly agreeable (*prosphoros*) division will be produced by completing a division into three ratios that exhibit the same sort of near-equality. If it were not so, in fact, serious doubt would be cast on the suggestion that it is of this formal feature that the tense chromatic's aesthetic sweetness is a reflection.

'When here too the greater ratios are put first in order,' Ptolemy goes on, 'there arises a tetrachord close to the tense diatonic, and more even (*homalōteron*) than it, both in itself and still more in association with the filling-out of the fifth' (38.21–3). Since the division's aesthetic effects are considered separately a few lines later, and since the analysis that intervenes is purely mathematical, it seems clear that the 'evenness' referred to here is at present conceived as a formal attribute. The division is 'more even' than the tense diatonic (whose ratios are $10:9 \times 9:8 \times 16:15$) because its ratios are more nearly equal in size, and form a smooth progression through successive epimorics from the greater to the smaller, $10:9 \times 11:10 \times 12:11$. This feature is accentuated, as Ptolemy says, in the 'filling-out' of the fifth, that is, when the division is placed immediately below the tone by which tetrachords are disjoined, so giving the sequence $9:8 \times 10:9 \times 11:10 \times 12:11$.

Ptolemy finds the impression made on the senses by this division distinctly appealing.

When a division is taken . . . on the basis of these numbers, the character that becomes apparent is rather foreign and rustic (*xenikōteron men pōs kai*

agroikōteron), but exceptionally gentle (*prosēnes*), and the more so as our hearing becomes trained to it, so that it would not be proper to overlook it, both because of the special character of its melody, and because of the orderliness (*to tetagmenon*) of its division. (38.29–33)

Thus the suggestion about the effect of such near-equalities, which was devised to fit the case of the tense chromatic, seems to be confirmed by its application to the new division. The perceptible counterparts of such formal evenness are the 'agreeable' character of the tense chromatic and the 'gentleness' of the new division, which is named as the 'even' (*homalon*) diatonic (39.6). It is not stretching interpretation too far to suppose that Ptolemy thought of these aesthetic characteristics as similar. It seems clear, too, that we are to treat the relation between formal evenness and perceived agreeableness or gentleness as self-evident; what is even in constitution makes a smooth, undisturbing impression on the senses, a contention that will fit well with the causal theory that dominates 1.3. Thus the (rather low-level) *hupothesis* that it is this formal attribute that is responsible for the impression can properly be regarded as one 'drawn from' perception, intelligibly connected with the phenomenon it is designed to explain. Even before it has been tested by experiment, the proposed explanation is not chosen arbitrarily.

Ptolemy's final remark about the even diatonic points forward to a new set of issues. One reason for accepting the division has been given.

Another reason is that when a melody is sung in this genus by itself, it gives no offensive shock (*proskopē*) to the senses, which is true, pretty well, only of the intermediate one of the diatonics among the others, the others being attuned by forcible constraint (*bia*) when taken by themselves, but capable of being successful in a mixture with the diatonic just mentioned, when those softer than it are taken in the tetrachords lower than the disjunctions, the tenser in those that are higher.'(38.33–39.5)

Three small points of detail need to be disposed of before we consider the general significance of these comments. First, the reference of the phrase 'among [or 'of'] the others', rather awkwardly placed in my translation, is ambiguous. The sense might be either that the 'intermediate' diatonic is the only other diatonic division that can readily be treated in the way described, or that it is the only one of all the other systems, regardless of genus, that can be so treated. Though the first reading of the Greek is marginally easier, the facts reported by Ptolemy in the sequel are fully consistent with the second and wider one. Secondly, as is made clear in the passage that follows, the 'intermediate' diatonic is the tonic diatonic of 1.15. Finally, the details of the framework within which some tetrachords are to be regarded as lower than disjunctions and some as higher cannot be unravelled without a study of the theory of *tonoi* set out

in Book II. But we can ignore these complications for the present, while we are still concerned only with divisions of the octave in which there is a single disjunction, one tetrachord lying above it and the other below it in a quite straightforward sense.

We have already seen that not all Ptolemy's derived divisions can acceptably be used in practice. Here we have our first indication of a second discontinuity between 'rational' systems and those of real music-making, and what he tells us is reported in no earlier source. From the point of view of theory, a single attunement is one in which all tetrachords are divided alike. It is only of such systems as these that the most fundamental of the rules of melodic sequence insisted on by Aristoxenus will hold, the rule that in any series of adjacent notes in an attunement, every note stands either at the interval of a fourth from the fourth note (inclusive) in order from it, or at a fifth from the fifth note in order.[11] The former relation holds when tetrachords are conjoined (so that the highest note of the lower is the lowest note of the higher), the latter when they are disjoined by the interval of a tone. But in most cases neither will be true when neighbouring tetrachords are divided in different ways. Aristoxenus unquestionably treated the rule as one that applies directly to the attunements of musical practice. For Ptolemy it will remain applicable to theoretically pure systems, but these and the systems of practice are not the same. The suggestion that it is only by 'forcible constraint' (a phrase echoed in a similar context at 74.11–12) that we can make ourselves sing in attunements that follow the same tetrachordal pattern throughout, except in certain special cases, shows clearly that such rationally ideal attunements are by no means aesthetically natural or pleasing. They give the ear an 'offensive shock'.

Ptolemy now fills in some of the details of the 'mixed' systems used in practice. The passage mentions the names given, presumably by performers themselves, to the various forms of attunement which they regularly use on stringed instruments, and explains how each is related to the 'theoretical' divisions he has analysed. We need not examine all the minutiae here. Some of them are in any case incomprehensible until the theory of *tonoi* is in place, and Ptolemy revisits them in II.16; we too shall have a little more to say about them later. For the present a mere sketch will be enough. Eight practical forms of attunement are identified. Three of them, Ptolemy says, are attuned throughout to the division of the tonic diatonic. Two have some tetrachords tuned in tonic diatonic and others in tense chromatic. Two more mix tonic diatonic tetrachords with those of

[11] See Aristox. *El. harm.* 53.33–54.11. The rule is the mainspring of the theorems of *El. harm.* Book III, and is often repeated in other authors.

tense diatonic; and one uses a mixture of tonic diatonic and soft diatonic tetrachords (39.6–14).

In II.1 Ptolemy provides an elaborate procedure to confirm these analyses empirically, by the use of one of his instruments; and in II.16 he adds further details, to be understood in the light of theoretical developments worked out in II.5–15. But even at this preliminary level, what he tells us is very striking. No other theorist gives an account of this sort, one that not only reveals a discontinuity between theory and practice, but also (given the additional information in II.16) provides a maximally precise mathematical description of each of the attunements in use. It is a relatively simple task to reconstruct these divisions on an instrument similar to those that Ptolemy describes; and if his analyses are near the mark, we can thereby recreate for our own ears a set of attunements used in Greek musical practice in the second century. For musical historians, then, this is an exceedingly precious document.

But the passage also has methodological repercussions. In the first place, this distinction between what is theoretically correct and what is practically usable seems to involve a difficulty which Ptolemy does nothing to circumvent or surmount. It is not hard to devise a plausible way of interpreting the relation between theory and practice, when the difference, as at 38.2–6, lies only in the fact that some theoretically proper divisions cannot acceptably be used. So long as those that are used are derived from rational principles in an orderly way, we can say that these principles govern the construction of all attunements that are rationally correct, including those which, because they are rationally correct, are accepted by the ear as musically well formed and beautiful; but that the ear has limitations, such that some of these attunements are beyond its powers of appreciation. Systems that can be heard as musical will then be a subset of those that are rationally well formed, and no Greek philosopher would have had any difficulty in accounting for the relatively limited scope of the ear's aesthetic discernment. No such explanation is possible in the present case, however. The fact that reason recognises the higher credentials of pure attunements while perception prefers its attunements mixed might indeed be accounted for by a development of ideas used by Ptolemy in I.1 (in particular, 3.14–19). But on Ptolemy's general approach, we shall still need to explain what it is about just these generic mixtures, and no others, that constitutes the formal counterpart of their aesthetic acceptability. No explanation is provided, and a large and uncomfortable gap is left in Ptolemy's exposition.

From another point of view, we must recognise that it is a rigorous application of his own methodological guidelines that has brought Ptolemy to these disconcerting conclusions. He promised to bring his

theoretical constructions to the judgement of perception, and he has done so, despite the apparent awkwardness of the results. No other Greek theorist had so directly confronted his theory with his observations of musical practice, and if Ptolemy too had chosen to duck this particular challenge, none of his rivals would have been in a position to criticise him for it. Again, the very fact that his accounts of the attunements used by musicians do not fit his theoretical analysis straightforwardly (and we shall shortly notice some other respects in which the fit is less than perfect) should increase our confidence in the reliability of his descriptions; they have not been 'rationalised' to suit the theory. All this is to his credit. We should also notice that while his account of these attunements raises difficulties for his general presuppositions about the consonance of reason and perception, in another way it serves to substantiate the basis of his theoretical divisions of tetrachords. If we can trust what he says in II.1 about the results of his elaborate experiments with instruments, the analyses he has provided for the attunements of practice are accurate to a hair. In that case, while the attunements mix systems together in a way that the theory cannot explain, the rational *hupotheseis* have turned out to be well justified, since it is only through the divisions determined by them, and not through those reached by alternative methods, that the tetrachords put together in the attunements of real musicians can be accurately described.

But this happy conclusion must immediately be qualified. The element that is constant through all the musicians' attunements is the division belonging to the tonic diatonic. It can be used on its own, or in various mixtures, but no attunement fails to include it. Yet it is precisely this division that was not derived in the regular way from Ptolemy's *hupotheseis*. New procedures, broadly consonant with the *hupotheseis* but not determined by them, were required to construct it. Hence the attunement that is at the heart of musical practice, essential to all its structures, is one that is anomalous by the standards of pure theory. We noticed earlier that Ptolemy is inclined to treat this version of the diatonic as the norm, and that he is prepared to use it as the source from which rules can be extracted about the proper or natural behaviour of diatonic divisions in general, even where other diatonic systems lack the features that these rules prescribe. The tonic diatonic plays this role in some of his criticisms of Aristoxenus and Didymus, and in the latter case falls under the heading of the 'simple' diatonic, the diatonic in its 'essential' form (see pp. 119–20 and 131 above). The divisions derived by an orderly procedure from the primary *hupotheseis* have apparently been relegated to the sidelines, while pride of place goes to one with a more dubious rational pedigree. I do not think we should even try to resist the conclusion that the principal reason

for Ptolemy's adoption of this division was his observation of its role in practice, and that the 'rational' explication he has given for it is an expedient of a post hoc and somewhat ad hoc variety, designed to fit what observation revealed. (We should recall that the criticisms of Aristoxenus and Didymus occur in passages where fault is being found not with their ignorance of rational principles, but with their failure to accommodate their divisions to the findings of perception.) If this is true, it is not reflected in the order of his presentation, where the rational considerations are developed first. But we may legitimately doubt whether Ptolemy would have found these considerations compelling if he had not known already that the facts of practice demanded that this division be recognised.

This movement of thought, from a fact of observation to the search for a way of representing it as conformable to reason, is still more clearly exemplified in the final part of 1.16. Here it is not even camouflaged as a derivation from *hupotheseis*, subsequently tested by empirical means. Ptolemy has identified two attunements used by musicians in which some tetrachords are in tonic diatonic and others in tense diatonic. This is so, he continues, 'except that while they sing in accordance with the tense diatonic that has been set out, . . . they actually tune [their instrument] to another genus, close to that one, but plainly different; for they make the two higher intervals tones [in the ratio 9:8], and the remainder, as they think, a half-tone, but as reason implies, what is called the *leimma* [in the ratio 256:243]' (39.14–19).

Though the ratios of Ptolemy's tense diatonic ($10:9 \times 9:8 \times 16:15$) are close to those of this division, Ptolemy probably had good evidence for his claim that it is to the latter and not the former that musicians tune their instruments. As I explained earlier, it is one that can be attuned very simply and reliably through the 'method of concordance' (see pp. 121–2 above); and Ptolemy alludes to the fact at 40.14–17. An inquisitive researcher can easily discover whether a musician, when setting up his attunement, is using this technique in a straightforward and unmodified form. If he is, the only diatonic division that will emerge from a careful application of it is the one mentioned here, $9:8 \times 9:8 \times 256:243$. The evidence of his eyes and ears, then, could have given good grounds for being confident that it is this division that the musicians were aiming at in the relevant tetrachords of these instrumental attunements. (The musicians' 'Aristoxenian' description of the small interval as a half-tone is neither here nor there.) A couple of remarks at 44.1–7 come close to clinching the case. There Ptolemy explains that musicians construct this division by the attunement of two successive tones, and that the small ratio at the bottom of the tetrachord arises simply as 'what is left'; and he

contrasts their procedure, as one that is 'easy', with the trickier one needed to construct the similar, but not identical division of the tense diatonic. The reason why the way in which they form their attunement is easy is, fairly plainly, that it is easy to attune the interval of a tone, downwards, for example, by moving from a given note through the interval of a fifth downwards followed by a fourth upwards. Since these intervals are simple concords, the ear recognises them reliably and with ease.

We may be more doubtful of his claim that they nevertheless sing the intervals of the tense diatonic. One might suppose that the effect of two slightly different forms of tuning simultaneously in play would be noticeably unpleasant, and that Ptolemy's motive is only to find a niche for one of his 'pure' systems. Ptolemy himself denies that the effect is obtrusive. 'This works for them well enough,' he says, 'since there is no noticeable difference between the ratios 9:8 and 10:9, or in the lowest positions between the ratio 16:15 and the *leimma*' (39.19–22); and he repeats a little later that 'no noticeable offence arises' (40.3). If there is no noticeable difference, however, one might well ask how Ptolemy knew that there was any difference at all. But it need not have been impossibly difficult. Even if the difference is unobtrusive in performance, the distinction is perceptible enough to be observed and identified by careful comparison of the singer's intervals with those of a prepared, experimental instrument, under 'laboratory conditions'.

It is not in fact unlikely that singers would have tended to use intervals approximating more closely to the tense diatonic division. There are various reasons for this. The most important in the context of purely melodic music is that in this division the relation between the highest note of the tetrachord and the third note is a major third in the ratio 5:4, a perceptibly smoother and sweeter relation than that of a true ditone in the ratio 81:64.[12] Rather similarly, a modern keyboard instrument is not tuned in a way that corresponds precisely to the intervals which a singer trained in unaccompanied performance will 'naturally' use; and so long as the instrument is not too dominating (a spinet or a small harpsichord, for instance, rather than a grand piano), the singer may continue to produce his 'natural' intonations even when an accompaniment is present. The singer may well be aware of the differences; the audience, usually, is not. It is at least possible, then, Ptolemy's account of the matter is correct.

[12] Compare the reason given by Aristoxenus for his contemporaries' preference for an interval slightly smaller than the true ditone, in the upper part of an enharmonic tetrachord, *El. harm.* 23.15–17. He attributes it to their unremitting quest for 'sweetness'. It is overwhelmingly likely that the distinction he has in mind is precisely that which a mathematical theorist would represent as holding between intervals in the ratios 81:64 and 5:4.

His initial attitude seems to be that the tuning used on instruments, though practically convenient, is nevertheless a perversion of what is theoretically correct. The performers are 'wrongly employing' the ratio 9:8 instead of 10:9 in the highest position and the *leimma* instead of 16:15 in the lowest (40.4–6).[13] Yet he immediately offers both practical and theoretical reasons for treating the division as legitimate. It is the theoretical reason that now concerns us. It is that 'the ratio of the *leimma* has a certain affinity with the fourth and the tone, marking it out from the other ratios that are not epimoric, since it follows inevitably when two ratios of 9:8 have been inserted into the ratio 4:3' (40.10–13). Hence he proposes to accept this genus (40.8–9), and gives it a title, the 'ditonic' diatonic (40.18–20).

This bit of argumentation is obviously designed to explain away one embarrassing feature of the division, its use of a non-epimoric ratio, which is flatly inconsistent with the rational *hupothesis* laid down at 33.5–9. There it is said that the notes of the tetrachord 'always' (*aei*) stand to their neighbours in epimoric ratios. Ptolemy's readiness to bend this absolute principle in order to accommodate the ditonic diatonic is surprising, and we are left to wonder whether there is any limit to the expedients he would allow, so as to account for recalcitrant facts of observation. If his explanation of the status of the *leimma* is accepted, however, it is not hard to imagine how the rest of the division would be justified. It shares with the tense diatonic and the even diatonic a version of the equality of division (two tones of ratio 9:8) which was found significant there – 'and still more in association with the filling-out of the fifth', as he said of the latter of these divisions (38.23).

Let us review the results of our discussions in this long chapter. In identifying the tetrachordal divisions that are to be counted as correct, in explaining their credentials and in marking the salient distinctions between them, Ptolemy has called on considerations of several different sorts, and by his own standards, different levels of persuasiveness. At the core of the rational *hupotheseis* is the doctrine assigning melodic intervals to epimoric ratios. As we saw earlier (pp. 79–87), it is one that can be defended not only for its coherent 'rationality', but also for its intelligible links with quite general features of perception and aesthetic sensibility.

[13] It is curious that Ptolemy also refers in this passage (40.6–8) to the attunement adopted by these performers for the enharmonic. His earlier remarks (and those of other authors in this period) indicate that the enharmonic was no longer performed or appreciated at all. Perhaps he is thinking of a division used by theorists or teachers who attempt to convey the structures of ancient systems audibly to their students, whether they are Aristoxenians or 'Pythagoreans' in the mould of Thrasyllus (Theon Smyrn. 92–3). But there is nothing in the text to suggest that he is referring to theorists, rather than to performing musicians.

Allied to this doctrine, however, is a thesis about the stages through which divisions are to be derived, whose status is allegedly rational, but which seems to rest on nothing more than Ptolemy's obsessive sensitivity to reappearances of the numbers 2 and 3. In addition, the process of derivation involves at least one rule, that assigning the smallest ratio to the lowest interval, which is warranted by perception alone. It is neither derived from formal principles nor shown to be the perceptible counterpart of any significant mathematical property of a system. Once all these principles are in place, one set of divisions can be derived from them by acceptable mathematical procedures alone.

Ptolemy asserts that perception too will recognise these divisions as perfect. But he immediately undercuts this claim by noting that not all of them are aesthetically agreeable. Doubts are thereby cast on the capacity of perception to carry out the tests required of it. The notion of rational acceptability is also being stretched. One division, the tonic diatonic, has already been accounted for anomalously, and two more that are not derivable from the *hupotheseis* will follow. The formal distinctions underlying the acceptability of the tense chromatic and the aesthetic significance of the *puknon*, like the explanations offered for the anomalous divisions, are all consonant in a general way with the kinds of consideration that determine the *hupotheseis*; but the *hupotheseis* provide no adequate justification for the roles they are given. In all these cases it is observation, in the first instance, which demands that the distinctions be recognised as significant and the divisions as correct. Ptolemy seeks to show that these demands are consistent with the relevant sort of rationality, but he cannot demonstrate that it requires them. Still less can he provide rational considerations to explain the predominance, in real music-making, of the theoretically anomalous tonic diatonic, or the particular characteristics of the generically mixed attunements which musical practice prefers.

The relations between rational and perceptual criteria, as Ptolemy deploys them, are thus substantially more complex than his reflections, at the opening of the work, would lead us to expect. The independence of certain perceptual rules from rational ones, in the principles from which the derivations begin, is not something for which those reflections prepared us. From that point on, we find a gradient from propositions very closely assimilated to the requirements of reason to ones that have little or no connection with them. The propositions that have the best rational credentials are said to be confirmed by perception, but that claim turns out to need qualification; there are perhaps two distinct varieties of perceptual assent, but Ptolemy does nothing to disentangle them. Next we have a group of propositions on whose truth perception insists and which can be made rationally intelligible, but which reason does not positively

require. Finally there are those that are again perceptually evident, which can be expressed in mathematical language and shown, sometimes with a bit of a stretch, not to be flatly inconsistent with the *hupotheseis*; but they remain, in the rational perspective, no more than arbitrary or accidental facts.

Ptolemy, I think, has striven conscientiously to live up to his method and his ambitious objectives. He can fairly claim to have succeeded in two important respects. He has adopted no theses that conflict with the principles of reason, as he understands them, and he has remained consistently faithful to the facts with which observation confronted him. What he has failed to show is that the demands made by reason and by perception match one another at all points, that every formal excellence has its aesthetic counterpart, and that every aesthetic intuition rests on mathematical foundations.

9 Larger systems: modulations in music and in method

By the end of Book I Ptolemy has completed his analysis of divisions of the tetrachord; but he takes one further step before moving on to a new topic. II.1 is occupied by an account of an alternative method of confirming the patterns of ratios attributed to the attunements of practical musicians in I.16. Here Ptolemy reverses his former procedure. Instead of first arguing to the values of the ratios on 'rational' grounds and then confirming the results by ear, he now begins by constructing the attunements by ear on the strings of an eight-stringed instrument, and then argues that the ratios of intervals constructed in this way must indeed have the values he has assigned to them. From here Ptolemy is led on to a discussion of certain other instruments that can be used for the same purpose; this occupies II.2. We shall review the contents of these two passages, among others, in Chapters 10 and 11.

The transition to a new phase of the investigation is clearly signalled at the beginning of II.3. 'Let that be our outline of what is scientifically understood (*ta theōroumena*) concerning the concordant and melodic relations between notes that are established in conformity with the lengths of string plucked, the homophones being included along with the concords. The next topic for discussion after these is that dealing with the *systēmata*' (49.4–7). The precise meaning of the word *systēmata* will be discussed below; for the present let us assume that it refers to scales extending over the range of an octave or more. The ramifications of this topic are pursued right through the remaining parts of the treatise that are concerned strictly with harmonics (II.3–III.2), but in the present chapter we shall consider only the contents of II.3–11. The passage is designed to develop by gradual steps the conceptual and practical basis of the structures known as *tonoi*, and to establish their main characteristics, their musical functions, their number and the relations in which they stand to one another.

From a musicological perspective, the subject of the *tonoi* is the most problematic of all the main departments of Greek harmonics, partly because of gaps in our evidence and confusion in the sources we have; but

these confusions themselves are due in large degree, I believe, to the fact that the single concept of *tonos* seems to have been developed to organise distinctions grounded in two subtly different kinds of musical practice. Few authors avoid inconsistency, since it is rare for them to identify the different musical functions commonly embraced by the term sufficiently clearly to allow them to exclude one or the other altogether, or to provide a coherent account of their interrelations. In this respect Ptolemy's discussion is unusually sharply focussed, but we shall need to ask whether the clarity of his position is bought at too high a price.

A couple of sentences from the end of the passage we are considering will serve to highlight the central methodological issue that will concern us in this chapter. In II.11 Ptolemy closes this phase of the discussion with the words: 'Let that, then, complete our exposition of a rational and adequate account of the seven *tonoi*' (66.3–4); and II.12 begins with the statement that 'the remaining task, in the enterprise of displaying with complete clarity the agreement of reason and perception, is that of dividing up the harmonic *kanōn* . . .' (66.6–8). It is natural to construe these remarks as exactly parallel to what was said towards the end of I.15: 'The fact that the divisions of the genera set out above do not contain only what is rational but also what is concordant with the senses can be grasped, once again, from the eight-stringed *kanōn* that spans an octave . . .' (37.5–7). The main question that will concern us is whether or not the conceptions of 'rationality' governing the divisions of the genera in Book I and the analysis of the *tonoi* in Book II are in substantial respects the same. The question cannot be answered unless the rather complex musicological agenda of the passage is tolerably well understood, and for that reason I have been unable to avoid including some rather lengthy slices of exposition among this chapter's discussions.

In II.3–5 Ptolemy addresses certain necessary preliminaries, exploring conceptions and developing terminological resources that will be used in the succeeding analyses. It will be simplest to begin from his definition of *systēma* in II.4: '. . . we may say that the name *systēma*, unqualified, is given to a magnitude put together out of concords, just as a concord is a magnitude put together out of melodics, and a *systēma* is, as it were, a concord of concords' (50.12–15). The nearest modern equivalent to *systēma* is 'scale', and in many writers the term is applied indiscriminately to long or short sequences of intervals. (It is usually reserved for sequences of three intervals or more, especially those like the tetrachord which have significant musical functions. Aristoxenus occasionally applies it to a sequence of two, as at *El. harm.* 29.1–6.) In Ptolemy's usage, only those sequences that are constituted by putting together two or more groups of intervals, each bounded by a concord, will count as *systēmata*. Thus a

single tetrachord, for instance, is not a *systēma*, but a *systēma* will be formed when two of them are placed in conjunction or disjunction. (In conjunction, the lowest note of the higher tetrachord is the highest note of the lower, and the concords put together to form the *systēma* will both be fourths. In disjunction the tetrachords are separated by the interval of a tone, and the *systēma* will be made up of a fourth and a fifth.) It would be fair to regard Ptolemy's definition as stipulative, since it is not reflected in common technical usage, but his decision to restrict the term's application in this way creates no immediate problems. The important point is that since the new subject has already been described as 'that dealing with the *systēmata*' (49.7), the definition explains its scope; and it also allows us to see the point of a set of distinctions drawn up, prior to the definition, in II.3, to which we shall turn in a moment.

Book I described each concord individually, and analysed the ways in which the smallest concord, the fourth, can rationally and musically be divided. The present phase of the investigation will study the ways in which these divided fourths, and the fifths made up of them together with the tone of disjunction, can be assembled to form *systēmata*; and it will raise issues about the ways in which these *systēmata* are related to one another. It is assumed that every significant structure greater than a fourth is an association of sub-structures bounded by concords; a sequence bounded by notes a sixth apart, for instance, could only be a fragment of a *systēma*. The assumption is not arbitrary. It reflects the facts of Greek practice; and it is theoretically underpinned by the contention to which Ptolemy returns several times in the present passage, that concords are 'prior' to the lesser intervals into which they are divided (see e.g. 62.9–11, 63.12–14). These smaller intervals acquire their musical functions only through their relations to the framework of concords in which they are contained.

Within this conception of *systēmata* the content of II.3 falls into place. It deals with the 'forms' or 'species' (*eidē*) of the primary concords, the fourth, fifth and octave. These are defined by the order in which the smaller intervals or ratios within the concord are arranged. Suppose, for instance, that two tetrachords in the tonic diatonic genus are placed together in conjunction, so that the sequence, from the top down, is 9:8, 8:7, 28:27, 9:8, 8:7, 28:27. There is a tetrachord divided in the familiar way between the highest note and the fourth note, and another between the fourth note and the lowest; but the intervals between the second note and the fifth, and between the third note and the sixth, are also fourths, put together from the same smaller ratios but in different orders. Ptolemy will call these different arrangements the first, second and third 'forms' of the fourth, depending on whether the ratio that would take the highest

Forms of the fourth

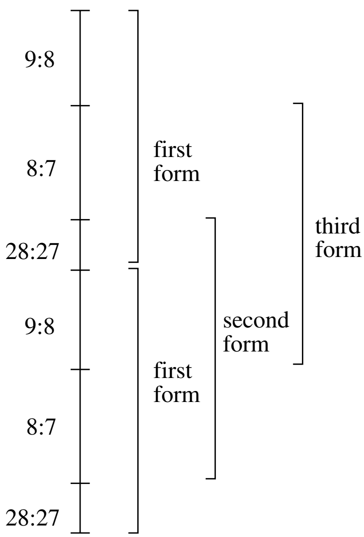


Fig. 9.01

place in a tetrachord between fixed notes, and is primarily responsible for the division's perceived 'softness or tenseness', comes first, second or third in order, reading from the top. In the present example the relevant ratio is 9:8, and the forms are those set out in Figure 9.01.

Every structurally significant interval of a fifth contains a tetrachord together with the tone by which tetrachords are disjoined, and each octave contains two tetrachords and a tone. Their constituent intervals can also be rearranged, in similar ways, and the arrangements are identified as 'first', 'second' and so on by reference to the location of the tone (49.9–13). It is important to realise that the reorderings of the fourth include only those that appear in different stretches of a sequence formed by the association of two tetrachords of the kind previously analysed. They do not include arbitrary reshufflings such as 28:27, 8:7, 9:8 (in tonic diatonic), since these will occur nowhere in a sequence put together from tetrachords of the regular sort. Similarly, in the re-orderings of the fifth and the octave, the additional tone is never to be inserted inside the boundaries of a regularly formed tetrachord, that which lies between fixed notes; its function is always to stand between such tetrachords. Hence the underlying order of the ratios will always be the same. Its alterations

depend only on the position of the segment we are considering, within a longer sequence of tetrachords or of tetrachords separated by tones.

It is then easy to see that there will be as many different forms of each concord as there are ratios or intervals within it: three forms of the fourth, four of the fifth and seven of the octave (49.17–19). Ptolemy goes on to identify those forms of each which are capable of lying between fixed notes; they are the first form of the fourth, the first and fourth forms of the fifth, and the first, fourth and seventh forms of the octave. These points are argued quite elaborately (49.19–50.10), though little use is made of them in the sequel.

Taken as a whole, the passage consists principally of definitions, and of statements of consequences that follow analytically when the definitions are taken together with certain propositions established earlier in the work. That is, once we know what a 'form' is, and how the labels 'first', 'second' and so on are to be assigned, then given the ways in which the concords have been represented in Book I, the rest will follow. An apparent exception is the group of propositions explaining which forms of each concord can lie between fixed notes. In order to derive them, we evidently need an account of the structure of the two-octave system within which certain notes are to count as 'fixed'. Here Ptolemy appears to be anticipating issues as yet unexplored, since the structure is not discussed in detail until II.5. But in fact he is assuming nothing that has not already been established. He presents his argument against the background of a system in which one pair of conjoined tetrachords is separated from another such pair by a tone. With the addition of a further tone at the bottom, this represents the two octaves of what is called the 'unmodulated' *systēma* in II.5; it is a framework standardly used by theorists to outline the overall structure of melodic 'space'. But for the purposes of the present argument, Ptolemy need not assume that this system is in any sense basic. We know already that the fixed notes are the boundaries of the tetrachords analysed in Book I (see 28.21–6). We know also that the system must be such as to display all the forms of the octave (since that is its theoretical purpose here), and this will be possible within a two-octave span only if each of its octaves is arranged in the same way. Each octave must be made up of two tetrachords and a tone, since the octave, by the principles of Book I, can be broken down in no other way; and the additional tone serves only as a bridge between tetrachords. Hence each octave can have any of three arrangements: tone, tetrachord, tetrachord, or tetrachord, tone, tetrachord, or tetrachord, tetrachord, tone. Which arrangement is chosen is unimportant, so long as it is the same in each of the octaves. Ptolemy's argument will still work, in fact, even if we break off the sequence part-way through a tetrachord, and relocate the intervals

removed from it at the opposite end of the system. Here again, then, despite initial appearances, the argument is again 'analytic', in the sense that it merely extracts necessary consequences of propositions that have already been adopted.

Given the definition of *systēma* that we discussed above, the serious point of II.3 is that in enumerating all the possible forms of the fourth, fifth and octave, it provides the materials for an analysis of all kinds of variation that are capable of determining the differences between *systēmata* (granted that we are currently putting on one side the differences generated by the various generic divisions of the tetrachord). In principle, since the octave is the sum of a fourth and a fifth, we could discover all possible variations of *systēmata* by considering only the forms of the fourth and the fifth, and the ways in which they can be linked with one another and with other instances of themselves. But a separate examination of the forms of linkage would be both cumbersome and unnecessary. Any acceptable concatenation of a fourth and a fifth will take one of the seven forms of the octave, and in all the forms of the octave, taken together, every form of the fourth and of the fifth will be displayed. Any sequence of two fifths will be contained partly within one octave of a given form, and partly in a second octave whose form must in principle be the same as that of the first, whether or not it is actually completed within the scope of the double octave. Hence the characteristics of any system composed of two fifths can again be analysed by reference to the forms of the octave. Two conjoined fourths are normally succeeded by a tone, which again completes the octave, and again it is the octave's forms that provide the most economical framework for analysis. A sequence of three fourths in conjunction occurs in one special kind of construction, and in this case the movement into the range of a second octave does not proceed in the way that it does in the first. But in II.6 Ptolemy argues that this is not a single *systēma* in its own right. It is a combination of parts of two different ones, each of which can be analysed by reference to forms of the octave in the usual way (see pp. 168–74 below). Hence all the *systēmata* formed in accordance with Ptolemy's definition, as sequences of concords, will be capable of being analysed by reference to octave *systēmata* and the various forms they take; and Ptolemy will later give further reasons (58.21–8) for treating the forms of the octave as the focus of his account.

Ptolemy does not offer arguments to support all the propositions outlined in the previous paragraph. But they are connected with the issues to which he turns in II.4. After defining the term *systēma* in the way we have noted, he next defines what it is for a *systēma* to be 'perfect' or 'complete' (*teleion*). 'The name "complete *systēma*" is given to that which contains all the concords, together with the forms proper to each of them, since in

general something is complete if it contains all its own parts' (50.15–17). He proceeds to argue, correctly, that in the context of the framework of tetrachords and tones that is presupposed throughout, the smallest *systēma* that can be called 'complete' in this sense is the double octave. Earlier theorists had admittedly called the octave 'complete', but if it is entitled to that designation it is not for the same reason; and the *systēma* of an octave plus a fourth had also been so described, but it, again, does not exhibit all the forms of the fifth or of the octave. Its status will be considered in II.6.

Ptolemy's insistence on reserving the title for the double octave is not just terminological pedantry. His argument serves as a justification for the theorists' standard assumption that the double-octave system is a sufficient framework for the whole of harmonic analysis. It is so because every form of the octave, fifth and fourth, and hence every kind of *systēma*, is contained within any properly formed sequence of notes whose end-points are two octaves apart.

> In the double octave, when two octaves are similar [in form] and are put together in the same direction, in every case, corresponding to every position in which the first of the disjunctions is placed, we shall find that all the forms of the octave, of the fifth and of the fourth are contained; and we shall find no further form in the concords that exceed the double octave. (51.12–16)

We made some use of this point above; and a moment's thought will show that it is true. Each octave is to be put together out of two tetrachords, and a tone which stands outside them. These three components can be taken in any order, so long as the order is identical in each of the two octaves. Then when the two octaves are placed end-on, one form of the octave will be bounded between the first note and the eighth, a second between the second note and the ninth, and so on up to the seventh and last form. The form of the final octave, between the eighth note and the fifteenth, will of course be the same as that of the first. Equally clearly, all three forms of the fourth and all four forms of the fifth will also be included. Since every *systēma*, as Ptolemy has defined the term, is a complex put together from a series of concords, we have reached the conclusion that all the forms that their components can take and all the ways in which they can be paired with one another are represented within the framework of the double octave. Harmonic analysis need concern itself with no larger framework.

In II.5 Ptolemy explains two ways in which names are assigned to the notes of the two-octave system. Both will be needed in the sequel. The account he gives is paralleled in no earlier source, and it quite probably constitutes his own attempt to reduce to workable order a terminology whose existing uses were ambiguous and potentially confusing. We need

nētē hyperbolaiōn
paranētē hyperbolaiōn
tritē hyperbolaiōn
nētē diezeugmenōn
paranētē diezeugmenōn
tritē diezeugmenōn
paramesē
mesē
lichanos mesōn
parhypatē mesōn
hypatē mesōn
lichanos hypatōn
parhypatē hypatōn
hypatē hypatōn
proslambanomenos

Fig. 9.02

not pursue all the details of his exposition. Each of the two octaves of the system referred to above may take any of seven forms, so long as the form of both octaves is the same. One method of naming, by 'position' (*thesis*), attaches a name to each of the fifteen notes simply by reference to its place in the series, regardless of the form of the constituent octaves. The names themselves were entrenched in the tradition; the lowest is *proslambanomenos*, the next *hypatē hypatōn*, and so on through the sequence set out in Figure 9.02.

The second method begins by assigning the same names to the same notes, in the case where the two octaves both have what we may call their 'basic' form, that is, where the tone that stands outside the tetrachords lies at the bottom of each octave. Ptolemy calls the double octave so constituted the 'unmodulated' (*ametabolon*) *systēma*. At the bottom is a tone; then comes a pair of tetrachords in conjunction, completing the first octave; and the second, identically structured octave begins from the highest note of the first. But in this second kind of naming, the names are not attached to the notes simply in virtue of their positions in the series, but in virtue of each note's relation to the pattern of tetrachords and tones; and the names are said to be assigned not 'by position' but 'by function' (*dunamis*). A note's 'functional' name is the name assigned to the note with that function in the unmodulated *systēma*, where the name given to each note by function is the same as the name it has by position.

A few examples will clarify the point. If we name the notes by position, the name of the eighth note from the bottom is *mesē*. In the unmodulated

nēte diezeugmenōn
paranētē diezeugmenōn
tritē diezeugmenōn
paramesē
mesē
lichanos mesōn
parhypatē mesōn
hypatē mesōn
lichanos hypatōn
parhypatē hypatōn
hypatē hypatōn
proslambanomenos = *nētē hyperbolaiōn*
paranētē hyperbolaiōn
tritē hyperbolaiōn
nētē diezeugmenōn

Fig. 9.03

systēma, *mesē* by position is the lower boundary of the higher of the two tones of disjunction, and that description identifies its 'function'. *Paramesē*, the ninth note by position, is the upper boundary of that interval. Let us now consider a different arrangement of the double octave, in which each octave is made up of two tetrachords with the disjunctive tone lying between them. We may conceive this as derived from the unmodulated *systēma* when the intervals of its highest tetrachord are shifted from the top of the series to the bottom. Plainly, the lower boundary of the higher disjunction will no longer be the eighth note from the bottom, but the eleventh (see Figure 9.03). If we name it by position, it is therefore *paranētē diezeugmenōn*. But if we name it by function, it is still *mesē*, since it still lies in the relation defined above to the series of tones and tetrachords. Similarly the twelfth note from the bottom, which by position is *nētē diezeugmenōn*, is by function *paramesē*.

Ptolemy explains in detail how the 'function' of each note in the system is to be defined at 52.14–53.10; but by now the principle should be clear enough. Two minor complications need to be mentioned. First, when segments of the system are rotated from one end to the other to generate different forms of the octave and double octave, the two ends of the system are joined together as if to form a circle, and the functions of *proslambanomenos* and *nētē hyperbolaiōn* will coincide in the same position. Hence the latter note, like the former, is conceived 'functionally' as lying immediately below a tone of disjunction. Secondly, in those *systēmata* in which the note that is *mesē* by function is less than eight notes from the

bottom, the disjunctive tone lying above functional *proslambanomenos* will reappear at or near the top of the double octave. But it is still conceived, for the purpose of identifying 'functions', as the lower of the two disjunctions; their designations as 'higher' and 'lower' are derived from their positions in the unmodulated *systēma*.

The point of the double terminology is to make it possible to distinguish a note's relation to the tetrachords and tones from its place in the sequence from bottom to top. When a form of the octave is moved to a different place in the two-octave series, it becomes convenient to say that a note with a certain 'function' has moved to such and such a new 'position'. It would no doubt have been less confusing to reserve the note-names for the functions, and to identify their positions merely by numbers, rather as we speak, for instance, of the first or the fifth 'degree' of a scalar series, and can distinguish these 'positions', at least in principle, from the 'functions' picked out by such words as 'tonic', 'dominant' and the rest. Ptolemy is constrained by the tradition; and so long as he is punctilious, when he names a note, in explaining whether the name is assigned by function or by position, the terminology, though cumbersome, is adequate to its task.

A couple of clarifying remarks complete II.5. Ptolemy points out, first, that the designation of certain named notes as 'fixed' and others as 'moveable' can apply only if the names are assigned by function (53.10–16). This is plainly so, in a context where the tetrachords and tones can shift their locations in the double octave. A note is not 'fixed', in the sense that it is a boundary of a tetrachord or of a disjunction, merely by standing at a certain position in the two-octave span. Secondly, he identifies the pairs of notes in the unmodulated *systēma* which are the boundaries of each of the seven forms of the octave, defining them as first, second and so on by reference to the position in them of the disjunctive tone (53.17–26). This requires no comment here.

Let us pause to take stock of the passage up to this point. It contains very little in the way of substantive doctrine; as I said in connection with II.3, the bulk of it consists of definitions, and conclusions derived logically from those definitions together with propositions that were established earlier. As such it is likely to seem harmless and uncontroversial, a set of explications and decisions about terminology that will be needed to facilitate the subsequent discussion, containing nothing likely to prejudice its results. The worst that could be said of it, then, is that it is all a little tedious. But there is more to it than that. The point is not that one might want to dispute Ptolemy's definitions, even where they are stipulative (as in the case of the term *systēma*), or to propose, for instance, a rival procedure for naming the notes. If Ptolemy chooses to use the terms in

nētē synēmmenōn	
paranētē synēmmenōn	tetrachord
tritē synēmmenōn	*synēmmenōn*
mesē	
lichanos mesōn	tetrachord
parhypatē mesōn	*mesōn*
hypatē mesōn	
lichanos hypatōn	tetrachord
parhypatē hypatōn	*hypatōn*
hypatē hypatōn	

Fig. 9.04

the ways he explains, he is entitled to do so; the usages are perfectly intelligible and coherent. Nor are there any serious flaws in the arguments he bases on the definitions and the other, associated propositions. But in selecting for explication just the concepts he does, Ptolemy is implicitly setting a particular agenda and determining in advance the questions about his subject, *systēmata*, which it will seem relevant to ask; and that is not a trivial matter.

In particular, his angle of approach throughout the passage, not excluding his discussion of note-naming in II.5, directs us towards a particular conception of the way in which one *systēma* differs from another, of what makes them different *systēmata*. Where their dimensions are the same, they will be different if and only if they differ in 'form', in the sense that has been defined, and more specifically in the forms of the fourths, fifths and octaves which they contain. Hence any theorist who sets off to distinguish *systēmata* in some other way, for instance by their relative pitches, is bound to appear misguided, since from Ptolemy's perspective he will be treating *systēmata* as different when they are 'really' just the same, merely located in different parts of the system. This will not matter, of course, unless the differences envisaged by the rival theorist have genuinely significant musical functions. But if they do, Ptolemy's strategy will inevitably obscure their significance, since the effect of his definitions will have been to make these differences conceptually invisible. Preliminary definitions of the terms and concepts considered relevant to an analysis are not after all necessarily harmless, if only because they depend on prior decisions (which Ptolemy does not discuss) about what will be counted as relevant. We shall return to these issues later.

On the face of it, II.6 is a digression. Ptolemy uses it to examine the credentials of what he calls the 'conjunct' *systēma* (*systēma synēmmenōn*); it is known by various other titles elsewhere. This *systēma* follows the course

of the unmodulated *systēma* through its lower octave, from *proslambanomenos* to *mesē*; but instead of proceeding next to a tone of disjunction it moves to a further tetrachord in conjunction, the tetrachord *synēmmenōn*. Thus three tetrachords are conjoined in a sequence, and the movement upwards from *mesē* does not repeat the opening steps of the lower octave (see Figure 9.04). Nor need any of the three highest notes of the tetrachord *synēmmenōn* lie exactly an octave above the eighth note in order below it; whether it does so or not will depend on the genus of the tetrachords.[1] By the standards of the structures so far considered it is therefore anomalous; crucially, it cannot be directly analysed in terms of the forms of the octave that it contains. Many theorists nevertheless recognise it as a genuine musical structure, and this view seems to rest on historical facts. There is evidence to show that attunements reflecting the main features of this system were used from early times.[2]

Ptolemy will not dispute the facts; but to maintain a consistent approach he needs to find a way of accommodating them without making this an independent kind of *systēma*, distinct from the others, since the differences between it and other *systēmata* do not fit into the pattern he has laid down. The 'digression' is of some importance to him, then, since if such an accommodation could not be found, he would be forced to reconsider the basis of his classification of *systēmata* by reference to the forms of the concords. But it is more than a digression in another way too. His account involves the introduction and preliminary analysis of the notion of modulation (*metabolē*), which will be of central concern in II.7–11. Here I shall offer first a sketch of his views about modulation as they appear in II.6 and parts of II.7 (it will need some revision later), and then an account of the way they are applied to the case of the conjunct *systēma*. This will equip us to pick up the questions about modulations of *tonos* that are raised elsewhere in II.7, and to pursue the answers given to them in the sequel.

In II.6 Ptolemy begins his discussion of modulation as follows.

In relation to what is in this sense called *tonos* there are two primary forms of modulation, one in which we go through a whole melody at a higher pitch or a lower one, keeping the sequence the same throughout, and a second in which not the whole of the melody is altered in pitch, but a part is altered in contrast to the original sequence. Hence the latter should be called modulation of melody, rather than of *tonos*. For in the former it is not the melody, but the *tonos* throughout the whole that is altered, while in the latter the melody is turned aside from its proper

[1] In the genera that Ptolemy recognises, *nētē synēmmenōn* will be an octave above *lichanos hypatōn* only in the tonic and ditonic diatonics; *paranētē synēmmenōn* will be an octave above *parhypatē hypatōn* only in the ditonic and tense diatonics; and *tritē synēmmenōn* will never lie exactly an octave above any other note.

[2] See for instance [Ar.] *Problems* XIX.47, Nicomachus *Ench.* ch.3.

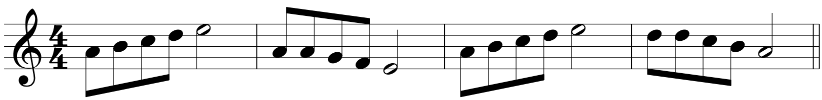


Fig. 9.05

ordering, while the pitch (*tasis*) is not altered as such, but as having an effect on the melody. Hence the former kind does not implant in the perception the impression of a difference in respect of function, through which the character [of the melody] is changed, but only of a difference in respect of height or depth of pitch. But the latter as it were expels the perception from the melody that is familiar and expected, when it has first strung together a coherent sequence at some length, and then changes in some way to a different form, either in respect of genus or in that of pitch – for instance, when it modifies the genus from continuous diatonic to chromatic, or when, beginning from a melody that has habitually made its transitions to notes concordant at the fifth, there occurs a change of course to notes concordant at the fourth . . . (54.12–55.15)

Let us immediately add a passage from the end of II.7.

For we shall not find that modulation with respect to *tonos* exists for the sake of higher and lower voices – as when whole instruments are raised or lowered in pitch, to accommodate that sort of difference, and no alteration in the melody results, or when the whole melody is completed in just the same way by lower-voiced or higher-voiced performers – but it exists in order that the same melody, in the same voice, starting sometimes from a higher position and sometimes from a lower, may produce a change in [the melody's] character. This is achieved, in shifts between *tonoi*, by the voice's limits no longer being attached to those of the melody, but one always ceases before the other, in one direction the limit of the voice occurring before that of the melody, and in the opposite direction that of the melody preceding that of the voice, so that the melody that was originally fitted to the compass of the voice, by falling short of it in one place in the modulations and exceeding it in another, provides for the ear the impression of a different character. (58.7–20)

There are difficult details here, but the general gist is tolerably clear. First, the kind of modulation with which Ptolemy will principally be concerned is one of those which, he suggests, might better be called 'modulation of melody'. It is not the mere transposition of the whole series of intervals in a tune to a different pitch-level. This latter procedure, which seems initially to be called 'modulation of *tonos*',[3] makes no difference to the char-

[3] The terminology is confusing. In the sequel Ptolemy regularly uses the phrase 'modulation of *tonos*', or a similar locution, in discussing the basis of the kind of modulation considered significant here, that is, one kind of 'modulation of melody'. This modulation of *tonos* is not identical with modulation of melody, simply as such. It is rather that while the transposition of a whole melody into a different *tonos*, what we might call a different 'key',

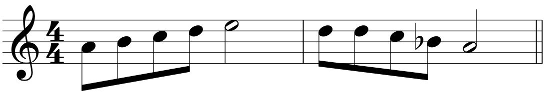


Fig. 9.06

acter of the melody, since there is no alteration in the 'functional' relations between the notes. (Thus if in the untransposed melody the first and second notes, for instance, were *mesē* and *paramesē* 'by function', they would have just the same functions in the transposed version.)

Ptolemy explains the notion 'modulation of melody' in a way that requires reference to the melody as it would exist without any modulations; 'the melody is turned aside from its proper ordering'. To clarify the idea, let me offer an example. The modern staff notation will not represent accurately the intervals of any Ptolemaic divisions, but the principles currently relevant will not be affected. Suppose we have a simple melody formed largely around phrases like the example in Figure 9.05.

In Greek terms, we can designate the notes we have used, according to their 'functions', as follows. The sequence from the lower E up to A is the tetrachord *mesōn*, from *hypatē mesōn* to *mesē*. The interval between A and B is the tone of disjunction between *mesē* and *paramesē*. The sequence from B to the higher E is the tetrachord *diezeugmenōn*, from *paramesē* to *nētē diezeugmenōn*. The tetrachords are divided according to a version of the diatonic genus. Plainly we could introduce a 'modulation of genus', in a Greek context, by altering the intervals within a tetrachord, in one of its occurrences, so that they conformed to a different generic division. But that is not the kind of modulation that concerns us here.

Consider instead what has happened, if after the relations involved in these phrases have been well established, the melody moves to the sequence shown in Figure 9.06.

The new phrase looks like a modest variation on bars 3 and 4 in our first excerpt. We cannot call on notions of genus to analyse it, since the alteration of B to B flat is not a shift in the position of a moveable note inside any tetrachord. Note B was by function a fixed note, *paramesē*, and the interval between B and A was a disjunctive tone, whose size cannot be

leaves the form of the melody unaffected, one of the two principal ways of producing a modulation of melody can be represented as involving the movement of just one segment of a tune into a different key – that is, a modulation of *tonos*. (The other device is to shift part of the sequence into a different genus; but this is not the focus of Ptolemy's attention.) The equation of *tonos* with key is in some respects misleading, as we shall see, but it will serve the purpose for the present.

changed. It has apparently disappeared. Instead, a tetrachord of the same genus as we found between the lower E and A, and between B and the upper E, has been placed, in the last bar of the phrase, between A and D. Hence there must have been not only a change of pitch from B to B flat, but a change in the functional relations into which this A enters.

For theorists who treat the 'conjunct *systēma*' as an independent construction, one running parallel to the two-octave system over part of its length, and always available as an alternative into which the melody may move, this poses no problems. From the note that is *mesē* by function, the series can progress upwards either through a disjunction to *paramesē*, or to the lower moveable note of the highest tetrachord of the alternative, conjunct *systēma*. But for Ptolemy, who denies that the conjunct *systēma* is an independent structure, this analysis is not available. In fact, when the note above it is only a semitone distant, this A can no longer count as *mesē* by function at all, since that is defined as the note lying immediately below the upper disjunction. As a consequence, the functions of B flat, C and D in the new sequence must also be different from those of B, C and D in the earlier phrases. What has happened, on Ptolemy's account, is that we have inserted into our melody a small segment belonging to a *systēma* with exactly the same structure as the original one, but whose functions are disposed across a different range of pitch. Specifically, we can treat the phrase as belonging to a *systēma* each of whose functionally designated notes is a fourth higher than its equivalent in the original *systēma*. In this second *systēma*, then, the note A will not be *mesē* by function, but *hypatē mesōn*, from which a tetrachord of the appropriate form runs upwards to its *mesē*, which is D. Hence, functionally speaking, the notes of the last bar are *mesē* (D), *lichanos mesōn* (C), *parhypatē mesōn* (B flat), *hypatē mesōn* (A); and from this point of view an 'unmodulated' version of this part of the melody, kept in the original *systēma*, would not be identical with bars 3 and 4 of our first excerpt, but with bars 1 and 2.

We can now see what Ptolemy means in the difficult last sentence of II.7, which I quoted above. He tells us that when such modulations occur, 'the voice's limits are no longer attached to those of the melody' (58.15–16). It is an odd expression, another of those which (I now think) I previously misconstrued.[4] In the light of our example, I now take it to refer to the fact that in such cases the voice does not move to the pitches which the melody would require if its note-functions were projected consistently onto the same *systēma*. In the example, if the melody were written out simply in terms of the functions proper to its notes, it would apparently require the voice to move to pitches lower in its range than it

[4] See *GMW2*, p. 332 n.60.

actually does in the modulated version.[5] In this modulated phrase, therefore, the 'limit of the voice' – i.e., the lowest pitch it reaches as it descends – 'occurs before that of the melody' (58.16–17), where the melody is conceived as constituted by the sequence of its constituent functions. In other modulations the reverse would happen (58.18).

Ptolemy's analysis of the conjunct *systēma* can now be quickly disposed of. It runs, as we saw, in the regular way from *proslambanomenos* up to *mesē*, but then adds a further tetrachord in conjunction, rather than proceeding through a tone of disjunction. But this attachment of a tetrachord in conjunction above *mesē* is exactly what was involved in the musical example we have been considering, and the new tetrachord (A, B flat, C, D) was identified as one belonging to a *systēma* located at the interval of a fourth above the original. Hence Ptolemy proposes to treat the conjunct *systēma* not as a new and independent construction, but as a combination of parts of two regularly formed *systēmata* a fourth apart. It is produced by a modulation. A similar sequence of three conjoined tetrachords can be formed by descending through the two highest tetrachords of the system, and then modulating to a tetrachord belonging to a *systēma* a fourth below the original, its tetrachord *diezeugmenōn*. Ptolemy argues all this out in close detail in II.6, but I think we have taken our exposition far enough.

Given this conception of modulation, Ptolemy's account of the way the conjunct *systēma* is to be analysed works well. He is in fact by no means the only theorist to regard it as arising from a kind of modulation.[6] He is also able to explain the apparently special status that its traditional treatment conferred on it, both by a historical hypothesis (56.3–10), and by reference to the peculiar excellence of the kind of modulation it involves. His account at this point in the text implies that we would expect the relations holding between D, C, B flat, A in our example to have held, instead, between E, D, C, B. Each of the latter notes is a fifth above its counterpart in the tetrachord below (A, G, F, E), while in the former sequence the interval is a fourth. Hence from this perspective the difference between what is expected and what is actually played or sung is a tone, rather than the fourth I suggested earlier.

Just about the finest modulation, unique in its function, is that which is like the one we discussed in taking, as the addition that makes the change, the interval of a

[5] We shall see below that Ptolemy also offers a different way of identifying the relations between the modulated section of a melody and its hypothetical, unmodulated counterpart. On that approach, the modulated passage would lie a tone below its counterpart, rather than a fourth above it. But the argument of the present paragraphs could readily be adapted to that view of the matter.

[6] See e.g. Cleonides *Eisagoge* 205.2–6, Aristides Quintilianus 16.24–17.2, 29.12–14.

tone, that by which the fifth exceeds the fourth. For in that it is common to the genera [since the tone of disjunction is unaffected by genus], the tone makes this modulation in all of them; in that it is different from the ratios inside the tetrachords it can thoroughly alter the melody; and in that it is well proportioned, in accordance with its constitution as the first of the melodics, it makes the transitions of the melody neither very large nor altogether insignificant. For either of these is difficult for the ear to distinguish. (55.22–56.1)

The first of the points Ptolemy makes here is straightforward. The second needs qualification, since a tone in the ratio 9:8 does occur in the tetrachords of some genera. But since it never stands at the bottom of a tetrachord, the movement from *mesē* upwards through the disjunctive tone to *paramesē* is always clearly distinguishable from a movement through the lowest interval of any tetrachord. The third point is particularly intriguing. Though Ptolemy appeals to the judgement of the ear, the thesis that the tone is the 'first' of the melodics belongs plainly to theory. The tone is not the first in the sense of being the largest, or of being that in which the difference between the terms of the ratio is the largest simple part of the smaller term. On the basis that we discussed at an earlier stage (pp. 79–87 above), an interval whose ratio has that property would be the easiest for the ear to grasp clearly and accurately. But in that sense the 'first' melodic must be the major third of 5:4. The tone can be conceived as first only for the reason that it is the one melodic interval that arises directly from relations between concords. Ptolemy consistently maintains that melodic ratios must be derived through 'rational' operations on concords, and the operation involved in generating the tone from the fifth and the fourth is evidently the simplest and neatest of all. But in order to hear a tone as such, or to construct one by ear, we do not have to perceive it as standing in that relation to the concords; Ptolemy himself made this point at 20.14–18. The special audible characteristics of the tone, to which Ptolemy draws attention in the present passage, are not due then to its perceived relations to the concords, since it need have none, but to its formal connection with them. If the reasoning is to make sense, this must be another case in which the perceived attributes of a relation are presented as being reflections of its unperceived, formal properties.

There is little else here, however, that seems to reactivate that aspect of the strategy of Book I. The contention that harmonic relations are 'rational' has taken a rather different turn. Ptolemy has set off to represent the differences between *systēmata* in terms of the forms of their constituent concords. He has eliminated, by a neat enough device, the one familiar *systēma* that seemed unamenable to such treatment; and he will shortly turn to the business of establishing how the various forms of *systēma* are related to one another within the compass of the double octave. None of this involves the alignment of specific perceptible attributes of *systēmata*

with specific formal ones, or not by itself, since no attempt is made to characterise the former. The project in which Ptolemy now seems to be engaged is the equally 'rational' but less ambitious one of reducing the phenomena to coherent order, of presenting what is perceived by the senses in a form intelligible to the mind, and fitting the various stratagems of musical practice into a systematic framework. This is part of what was involved in Book I, of course, but by no means all. The project might be construed as a preface to a further exercise of the more ambitious sort, one in which Ptolemy would assign to each distinguishable *systēma* – each *tonos*, as he will call it – a distinct perceptible character, and would show how that character can be accounted for by reference to the intelligible form proper to the *tonos* in question. There are hints of such an idea much later in the treatise (III.7, III.12), but they are rudimentary.

There are issues in II.6 that call for further exploration, but at this stage we shall move on to consider the contents of II.7–11. Ptolemy will have a good deal more to say, and to imply, on the subject of modulation between *tonoi*. So far, we have treated these modulations rather as if they were transpositions of part of a melody to a different level of pitch, that is, roughly speaking, into a different key. This representation of them will turn out to distort Ptolemy's intentions, and the distortion will be important both musically and methodologically. But the adjustments we shall need are best left to emerge gradually, as we proceed.

The general programme of II.7–11 is straightforward. In II.7 Ptolemy raises three central questions about the *tonoi* and their interrelations, and each of II.8, II.9 and II.10 provides an answer to one of them. Since the answers conflict sharply with those presupposed in the constructions of certain other theorists, II.11 is devoted to an exposure of some of their errors.

When the word *tonos* first appears in Ptolemy's text (54.11, with the sentence that follows to 55.3), the difference between any two *tonoi* seems to lie in their pitches and nothing else. They are *systēmata* containing identical sequences of intervals, but occupying different ranges of pitch. The same conception is clearly implied at the beginning of II.7. Modulations can involve

> shifts in complete structures, to which we give the special name *tonoi* because it is from pitch [*tasis*, etymologically cognate with *tonos*] that they take their differences . . . for a *tonos*, in this usage, differs from a note only in that it is composite while the other is incomposite, like the difference between a line and a point, where once again nothing prevents us from moving either the point alone or the whole line to adjacent positions. (57.13–19)

If it is 'from pitch that they take their differences', and if the movement of a *systēma* from one pitch-range to another, as one might move a line

through geometrical space, constitutes change of *tonos*, then there seems to be no reason to resist the equation of *tonos* with key, or rather, with a *systēma* or scale in a particular key. No doubt the identity of a Greek key would depend more on its musical context than on its absolute pitch; but a degree of relativity in the conception does not alter its principal content. The remark that I cut out of the quotation above, that the quantity of *tonoi* is 'potentially infinite, as is that of the notes' (57.15–16), gives further fuel to this interpretation, since it is only if they are regarded as pitches, rather than as 'functions', that the notes can be said to be potentially infinite in number; and the same must apply to the *tonoi*. Up to this point, then, it seems not only possible but likely that Ptolemy means to identify a *tonos* with a *systēma* at some particular pitch-level, in some 'key'.

Though the number of *tonoi* is potentially infinite, 'in the actuality available to the senses it is determinate, since that of the notes is too' (57.20). What limits the number of notes is the number of melodic functions that there can be in a 'complete' *systēma*. For Ptolemy, the *systēma* will have to be 'complete' in the sense explained at 50.12–23, and it is of course the double octave. We must expect that the various different *tonoi*, too, are all to be represented within that framework; and in view of the remark just quoted, we must expect also that their number will be determined in some way by the number of notes. Hence it will be determined indirectly by the functions by which the notes are defined. But the relation, as Ptolemy understands it, does not link notes with *tonoi* as one to one. The number of *tonoi* will be neither that of the notes in the double octave he has set out (i.e. fifteen), nor even that of notes in the octave (eight).

There are in fact, Ptolemy asserts, three 'determinate limits' on the *tonoi*. There is a determinate ratio between the pitches of those *tonoi* that are furthest apart, a determinate number of *tonoi* between those boundaries, and determinate differences between the pitch of each *tonos* and those of its neighbours (57.21–4). Hence there are three questions to be asked. How far apart are the outermost *tonoi*? (This is considered in II.8.) How many *tonoi* are there? (II.9.) What are the sizes or ratios of the intervals between them? (II.10.)

These determinate features of the set of *tonoi*, Ptolemy says, are somewhat analogous to those of the concord of a fourth. Its boundaries, too, stand to one another in a definite and constant ratio, that of 4:3; its span is divided into just three smaller ratios; and the sizes of the ratios, though they differ from genus to genus, are nevertheless not indefinitely variable, and are determined by fixed principles (all this is a legitimate expansion of 57.25–7). But there is an important difference. Whereas in the case of the fourth, the three kinds of quantitative determinacy rest

on different foundations from one another, 'in the *tonoi* the other two follow in a way upon the first, constrained by one and the same restriction' (57.27–9). That is, the same principle that determines the answer to the first question will also determine the answers to the others. Hence from a 'rational' perspective the system of *tonoi* will be very tightly coordinated.

Ptolemy proceeds to castigate other theorists for ignoring this intimate connection between the three issues. They 'do not grasp the consequence of this restriction, and they set out each of the limits in ways that disagree with one another' (57. 29–58.1); and 'more recent writers' are constantly trying to outdo their predecessors by expanding the range between the outermost *tonoi*.[7] This pursuit of novelties, Ptolemy says, is 'inappropriate to the nature of attunement and to its periodicity (*apokatastasis*), by which alone one must determine the interval between what are to be the outermost *tonoi*' (58.3–5). Some sentences we glanced at earlier are adduced as evidence for this claim; they are to the effect that modulation of *tonos* is not designed merely to transpose a melody to a different pitch, but to produce a change in its character by shifting part of it to a different level (58.7–20).

What these considerations have to do with the thesis that the distance between the outermost *tonoi* is determined by the 'periodicity' of attunement is not immediately clear. But the concept of periodicity or cyclic recurrence is reintroduced in the opening sentence of II.8, the chapter in which the size of this distance is to be settled.

> Let us agree that the first and most important cyclic recurrence of similarity in attunement is, once again, in the first of the homophones, that is, in the octave, whose bounding notes are no different from a single note, as we have shown. And just as those of the concords that are put together with it produce the same result as they would produce if they were alone, so each melody that spans just the interval of the first homophone, or an interval produced from the first homophone by combination [i.e., some exact number of complete octaves], can run through its course in exactly the same way taking either of the outermost notes as its starting point. Hence in modulations of *tonos* too, when we want to move to one an octave higher or lower, we shall not alter any of the notes (whereas in all the remaining modulations we always do alter some), and the *tonos* itself remains the same as the original one. (58.21–59.2)

7 58.1–3. These 'expansions' are presumably designed to accommodate newly postulated additional *tonoi*. This interpretation is supported both by Ptolemy's own comments on theorists who make the range too great and the number of *tonoi* too large (in II.8, 9, 11), and by the regular attribution in other sources of thirteen *tonoi* to Aristoxenus and fifteen to some later theorists. Equivalent notes in the outermost of the thirteen are an octave apart, and they are an octave and a tone apart in the outermost of the fifteen. See e.g. Aristides Quintilianus 20.5–9, and cf. Theon Smyrn. 64, Cleonides *Eisagoge* 203.5.

Ptolemy elaborates these points through the rest of II.8. Their consequence is that anyone who restricts the range between the outermost *tonoi* to an interval smaller than the octave 'cannot have completed the cycle of attunement', while those who extend it beyond the octave are postulating redundant *tonoi* identical with earlier ones (59.6–12). In fact even those who treat as a separate *tonos* the one at an octave from the first have gone too far, since this *tonos* will be the same as that from which they started. Ptolemy pursues this thesis at a little length (59.12–20), which is interesting, since he is himself committed to the view that it is the octave that bounds the sequence of *tonoi*. We shall return to this point in a moment.

On the evidence of this stretch of text it would still be just possible to construe Ptolemy's *tonoi* as identically formed scales in different keys. We ourselves, after all, do not treat a major scale beginning on middle C as being in a different key from one starting on the C below. The consideration governing Ptolemy's arguments here is that of the opening of I.6. There he claimed that

> it is always true of the concord of the octave, whose constituent notes do not differ in their function from a single note, that when it is attached to one of the others it keeps the form of the latter unaltered . . . And if one takes a note that lies in the same direction from both the extremes of the octave, downwards from both of them, or again upwards, as it is to the nearer of them so it appears to be to the further, and it has the same function as has that one. (13.3–10)

But on the basis of what was said in II.6 and II.7, where *tonoi* seemed to be distinguished only by their pitches, this would leave Ptolemy with some explaining to do. A scale starting on the higher C does not sound exactly the same as one starting on the lower. The difference in their pitches is plainly discernible, and if pitch determines *tonos*, they should be different *tonoi*. Ptolemy denies this conclusion. A *tonos* an octave from another 'remains the same as the original one' (59.1–2). What is it, then, that makes two *tonoi* the same or different?

A clue to the answer is already given in the passage from I.6 that I quoted above. Ptolemy twice suggests there that the apparent identity of notes an octave apart is related to their identity of function. He does not explore the notion at this stage; the concept of *dunamis*, 'function', as it applies to notes, is not unravelled until II.5. Immediately after his assault, in II.8, on those who posit a new *tonos* at the octave from the original, he offers an argument drawing on the same conception. 'The functions in the octave should not be measured by the quantity of its terms, but by the quantity of the ratios that jointly constitute it' (59.20–22). That is, there are eight notes in an octave, but only seven intervals or ratios; and it is by the ratios that the functions of the notes are determined. The note at the top of the octave is functionally identical

with the one at the bottom, just as was said in I.6, since the ratios in which either of them stands to the notes arranged in sequence on each side of them are exactly the same.[8]

On the basis of the present argument, Ptolemy will be able to insist that his *tonoi* are 'bounded by the octave', and yet are only seven in number. The boundary at the octave is needed to delimit the seventh interval or ratio; but it is not the beginning of an eighth *tonos*. But if it is the fact that there are seven ratios in the octave which determines that there are seven *tonoi*, as will shortly become clear, the differences between them must be those that arise from their taking, as their initial step, one or another of the seven ratios. In that case it will be the sequence of ratios in a *tonos*, rather than its pitch, that makes it the *tonos* it is; its identity is constituted by the 'form of the octave' that it contains. No other consideration can account for Ptolemy's insistence that the number of *tonoi*, and the distance between the outermost, are determined by the number of ratios in the octave and the functions they define.

This conclusion is strongly indicated by the closing sentences of II.8, and amply confirmed in II.9–11. The *tonoi* are not identical with the forms of the octave, however; they are, one might say, more concrete than that. They are sequences of notes and intervals, *systēmata*, whose structures are so constituted and interrelated that – within a particular range of pitch whose identity we shall consider shortly – each exhibits the octave in a different one of its forms. Hence though Ptolemy repeats several times that there must be the same number of *tonoi* as there are of octave-forms (e.g. 60.2–3, 61.2–5, 64.16–18), he never says that a *tonos* is the same as a form of the octave. Most revealingly, he argues in II.11 that if more *tonoi* than seven are posited by locating others between the existing seven, the sequence of functions in some adjacent *tonoi* will turn out to be the same, since there are no new sequences to be found. Hence a shift from one member of such a pair of *tonoi* to the other will produce no change in *tonos* at all, since the functions of the notes will not have altered (65.19–66.1). There will have been no change of *tonos*, he concludes, because 'the *tonos* will not even seem to be different in form from the previous one, but will be the same again . . . merely sung at a higher or lower pitch' (66.1–3; we shall reconsider the passage shortly).

Alteration of pitch, then, does not constitute alteration of *tonos*. Each distinct *tonos* must have a different 'form', corresponding to one of the

[8] Hence the implication of II.15 that corresponding notes in the two identical octaves of the double-octave system have different functions, accounting for their different names, is by present standards only a convenient fiction, designed to help us describe the various forms of octave mapped onto different stretches of that system.

forms of the octave. The conclusion should not be unexpected. *Tonoi* are varieties of *systēma*; and Ptolemy has shown a disposition, from the start, to treat *systēmata* as different only if they differ in the forms of their constituent concords. A set of *systēmata* that displays all the seven forms of the octave is bound to display all those of the fourth and the fifth. Hence a set of seven such *tonoi* will be the set of all the distinct kinds of *systēma* that there are. (Sequences longer than the octave will always be analysable either as repetitions, partial or complete, of the sequence proper to some one *tonos*, or as involving a modulation from one *tonos* to another.)

In order to reach this conclusion we have been jumping around in the text of II.9–11; and if we now return to the beginning of II.9, we shall find that we have anticipated the answer to the question it sets itself. The number of *tonoi* is seven. As Ptolemy said, the three questions he raised in II.7 are closely connected; and our attempts to get to grips with the first, that relating to the distance between the outermost *tonoi*, has indeed involved an answer to the second. But the second and third are also linked. The question about the number of *tonoi* is not wholly detachable from the question about the sizes of the intervals or ratios between them. Hence a good deal of II.9 is equally relevant, perhaps more relevant, to the question raised in II.10, the question how the ratios between the *tonoi* are to be established.

We cannot sensibly approach this question, or the way in which it is related to the previous two, without first deciding how it is to be understood. If the *tonoi* differ from one another by containing different forms of the octave, why should they differ in pitch at all, and so be separated by intervals or ratios? This takes us back to Ptolemy's earlier remarks about *tonoi* in II.6 and II.7, which seemed to suggest that they differ *only* in pitch, and can be construed as identically formed *systēmata* set in different keys. We can now see that this way of conceiving them misrepresents Ptolemy's intentions, but he persists in regarding them as lying, in some sense, in different ranges of pitch; and unless he is to be convicted of mere inconsistency, the implications of the earlier passages cannot be simply dismissed.

Let us recall Ptolemy's two different ways of naming notes, by position and by function (II.5). To name a note by position is, in effect, merely to identify its place in order in the two-octave *systēma* currently in play, and the names can be replaced by numbers without any real loss. To name it by function is to identify its relation to the sequence of tetrachords and tones; and since these can be shifted to generate different forms of two-octave sequence, the same function will appear in different positions, depending on the point in the sequence of functions from which a given double octave begins. Figure 9.07 shows two such double octaves, the first of

	systēma A		*systēma* B
15	*nētē hyperbolaiōn*	15	*paranēte hyperbolaiōn*
14	*paranētē hyperbolaiōn*	14	*tritē hyperbolaiōn*
13	*tritē hyperbolaiōn*	13	*nētē diezeugmenōn*
12	*nētē diezeugmenōn*	12	*paranētē diezeugmenōn*
11	*paranētē diezeugmenōn*	11	*tritē diezeugmenōn*
10	*tritē diezeugmenōn*	10	*paramesē*
9	*paramesē*	9	*mesē*
8	*mesē*	8	*lichanos mesōn*
7	*lichanos mesōn*	7	*parhypatē mesōn*
6	*parhypatē mesōn*	6	*hypatē mesōn*
5	*hypatē mesōn*	5	*lichanos hypatōn*
4	*lichanos hypatōn*	4	*parhypatē hypatōn*
3	*parhypatē hypatōn*	3	*hypatē hypatōn*
2	*hypatē hypatōn*	2	*proslambanomenos* = *nētē hyperbolaiōn*
1	*proslambanomenos*	1	*paranētē hyperbolaiōn*

Fig. 9.07

which, *systēma* A, follows the arrangement of the 'unmodulated' *systēma* of II.5. The positions of the notes are indicated by numbers; the note-names indicate functions. It should be emphasised that the sequence of 'positions' implies, by itself, nothing about the sizes of the intervals separating them. These will alter with the functions to which they correspond.

From one point of view, what we have here is a pair of identically formed *systēmata*, each running upwards from functional *proslambanomenos*, of which the first begins at position 1 and runs through a two-octave compass, while the second, if we temporarily ignore its lowest note, begins at position 2 and falls short of the double octave by one interval. From this perspective they differ in pitch, and one might consider them as representing sequences in different keys. But the natural way of depicting that scenario would be to complete the double octave of *systēma* B at the top, and to eliminate the interval below its *proslambanomenos*. If we also restricted each of our *systēmata* to its own two-octave span, this would make unnecessary the identification of *proslambanomenos* with *nētē hyperbolaiōn*, and the appearance of *paranētē hyperbolaiōn* at both the top and the bottom of *systēma* B. Ptolemy's exposition, however, contains no hint of this strategy.[9]

[9] See for instance Aristides Quintilianus I.11, noting particularly the reference to the 'wing-shaped' diagram of the *tonoi*, 23.25–24.1. A reconstruction of the diagram, which is missing from the MSS, is offered at *GMW2* pp. 428–9. This way of regarding the matter governs the arrangement of the *tonoi* in the Greek systems of notation, as recorded in Alypius *Eisagoge*, and as they appear in the surviving scores.

Turning now to the issue of the forms of the octave, it is clear that the form represented in *systēma* A between any two positions eight notes apart is different from the form exemplified in the stretch of *systēma* B that lies between the same numbered positions. In dealing with this matter at the beginning of II.11, Ptolemy concentrates on the octave between positions 5 and 12. His procedure for identifying forms of the octave works by reference to the location of the disjunctive tone; when the tone is the highest interval in the octave, the octave has the first form, when it is second from highest it has the second form, and so on (49.9–16). Thus in *systēma* A the octave between positions 5 and 12 is of the fourth form, and in *systēma* B it is of the third.

This points to the second way of conceiving the relation between the two *systēmata*. Instead of treating them as projecting the same sequence of intervals onto different ranges of pitch, we hold constant the range of pitch considered, and think of the *systēmata* as projecting onto it different sequences of intervals. Specifically, where the range considered spans an octave, they project onto it different forms of the octave. If our two *systēmata* represent two *tonoi*, and if *tonoi* differ only in so far as the forms of the octave they contain are different, it must be this second conception of the relation between the *systēmata*, and not one based on a notion of key, that is uppermost in Ptolemy's mind. That is, to give the link between *tonos* and octave-form an application, we must think of every *tonos* as inhabiting the same range of pitch. It is because the range must be held constant, as it cannot be in a system of keys, that *systēma* B does not extend upwards far enough to complete the series of functions in the order they have in *systēma* A. The 'missing' note and interval reappear, instead, at the bottom, so that one end of the sequence circles round to join the other, and the functions of *proslambanomenos* and *nētē hyperbolaiōn* coincide.

As we have seen, however, Ptolemy still wishes to regard one *tonos* as being in some sense higher or lower in pitch than another; and he is proposing to determine the sizes of the intervals separating them. But this does not involve a surreptitious return to the notion of *tonos* as key, though there are affinities between them. It is true that his exposition is a little confusing, since the pitch-relations between the *tonoi* are established in II.10, while the exact sense in which they are so related is not made quite clear until the beginning of II.11. But the idea is very simple. Every functionally designated note in *systēma* A will differ in pitch from its functional counterpart in *systēma* B by the same amount. This should be clear from a glance at the diagram; whatever the interval may be between the *proslambanomenoi* of the two *systēmata*, the same interval will separate any other pair of corresponding functions. Since Ptolemy chooses to select

the octave between positions 5 and 12 to identify the forms of the octave projected onto a given range by the various *tonoi*, he takes as his point of reference a functionally designated note, *mesē*, which – as it turns out – will appear in that range in every *tonos*. The difference in pitch between any two *tonoi* is thus conceived, straightforwardly, as the difference between the pitches of their respective *mesai*.

This rather lengthy account, in which I have not attempted to keep to the order of Ptolemy's presentation or the details of his arguments, was prompted by the need to explain how the *tonoi* can be thought of as located at different pitches, while the essential differences between them are nevertheless grounded in the differences between forms of the octave. We are now equipped to consider his account of the pitch-relations themselves; and in returning to the text of II.9–11 for this purpose, we shall also review some of the ways in which he himself argues for the conception of *tonos* that I have outlined.

Issues about the intervals between the *tonoi* emerge out of the question of the number of *tonoi* in II.9. Ptolemy has drawn an analogy. If one divided the fourth into some number of parts other than three, or constructed these parts with random sizes, 'both reason and perception would immediately object' (60.9–61.1). He continues:

> In the same way, since the *tonoi* contained in the octave correspond to the nature of the concords and take their origin from them, so that the *systēmata*, taken as wholes, may have differences that are concordant, if people seek either to make them more in number than the seven forms and ratios of the octave, or to make the differences between all of them equal, we must not agree with them; for they have no persuasive reason to offer, either for the equality of the augmentations between one whole *tonos* and another – such a thing being condemned as wholly inappropriate in harmonic attunement – or for the claim that all the differences are tones, for example, or again semitones or dieses, from the adoption of which they determine the number of the *tonoi* in accordance with the quantity of them that makes up an octave. (61.1–11)

The argument immediately developed out of these remarks is designed to undermine the notion that the proper way of relating the *tonoi* is to separate them by equal intervals. That principle could not settle the issue even on the assumption made by 'these people' (61.11–12; they must be Aristoxenians), that the octave can be divided into equal segments, since on that basis the *tonoi* could be separated by tones, half-tones, third or quarters of a tone, and nothing has been done to determine which spacing is correct (61.11–20). And in fact, as we know, the ratio of the octave cannot be divided into seven, or any number of equal ratios of integers at all. Hence the principle of equal division cannot even be applied; and the proposition that the divisions must be unequal is obviously

unhelpful by itself. Hence considerations of equality and inequality, as such, can give us no answer to our question (61.20–62.2).

Ptolemy's answer has already been hinted at in the lines quoted above, at 61.1–4. The *tonoi* 'correspond to the nature of the concords and take their origin from them, so that the *systēmata*, taken as wholes, may have differences that are concordant.' The sense of this slightly enigmatic statement is carefully explained at the end of II.9 (62.2–15), and its consequences are set out in detail in II.10.

If we return for a moment to the *systēmata* represented in Figure 9.07, we might be forgiven for assuming that *systēma* B differs in pitch from *systēma* A, in Ptolemy's sense, by the interval which lies between *paranētē hyperbolaiōn* and *nētē hyperbolaiōn*, whatever that may be in a given genus. In that case, the sequence of intervals by which successive *tonoi* are separated would be that of the intervals of an octave *systēma* in the genus in question; and they would be different in different genera. Such an arrangement might be said, in an extended sense, to 'correspond to the nature of the concords', since it would be determined by the ways in which the fourth can properly be divided to form tetrachords. But it would be complex and confusing. No Greek theorist adopts it, so far as I know, and certainly Ptolemy does not. From a musical point of view, the consideration that governs his scheme is that 'the *systēmata*, taken as wholes, may have differences that are concordant'. What this turns out to mean (62.2–15) is that from a given note in any *tonos*, designated by its function, it must be possible to reach the corresponding note in every other *tonos* by a sequence of moves through concordant intervals only. If the spacings between the *tonoi* corresponded to the sequences of intervals in each genus, this principle could not be observed. The only note that can be reached by a series of steps through ratios of concords is one that stands to the original note either in the ratio 9:8, or in the ratio of the *leimma*, 256:243, or in a ratio produced by some combination of such tones and *leimmata*. Greek writers often remark, as Ptolemy does here, that the best and most acceptable kind of modulation is through a concordant interval.[10] Even in modern 'classical' music, the modulations of key that seem most natural are those to the keys of the subdominant or the dominant of the original key, that is, to those keys whose tonics lie at a fourth or a fifth from the original tonic.

This, then, is a fact about musical practice and the ear's perceptions. Ptolemy offers it a theoretical interpretation. 'It is essential not only here, but everywhere, that the homophones should take precedence over the concords and be adopted as principles prior to them, and that the

[10] See e.g. Cleonides 205.10ff., Aristides Quintilianus 22.15–18.

concords should have the same priority over the melodics' (62.9–11). Thus we found in I.7 and I.15 that the aesthetic virtues of the concords are derived from their relation to the finest of all intervals, the octave, and those of the melodics from their relation to the concords. Correspondingly, the concords have the ratios they do because they divide the best of the ratios, 2:1, in the most 'rational' way, and those of the melodics are found by divisions of the smallest concord according to consistent and intelligible principles. 'One should not construct the concords out of the melodics, but conversely, the melodics from the concords' (63.12–13). As applied to the generic divisions, this would mean that one should not try to establish the ratio of the fourth by putting together melodic ratios that were independently assumed to be correct, but establish the ratios of the melodics by dividing the 'better' ratio, belonging to the 'finer' interval, on a rational basis. In the present context, there is no need to proceed at the level of divisions of the fourth at all. Adjacent *tonoi* will of course be spaced at 'melodic' intervals, in the sense that they are harmonically acceptable intervals smaller than the fourth. But the spacings can be found at a higher level of analysis, simply by using the concords to create divisions within the octave. Musical practice presupposes that modulations through concords are best. It also assumes that every *tonos* can be reached by the best sort of modulation from some other, and that the whole set of *tonoi* is linked together by modulations of that sort; that is, a first *tonos* is related in this way to a second, the second to a third, and so on, until the whole series has been completed. Ptolemy's remarks are designed to represent these intuitions as intelligible at a formal level, and so to account for them.

The details of the arrangement are worked out in II.10. Ptolemy admits, rather grudgingly, that his conclusions are not entirely new. Where he claims originality is in the rationality of his method of establishing them. 'The people who go up to eight *tonoi*, by way of the one included superfluously in their number along with the seven, seem to have stumbled, somehow or other, on the differences that are appropriate to them, but not on the basis of the proper approach' (62.16–18). The details of the procedure by which, so Ptolemy alleges, they arrived at their disposition of the *tonoi* need not concern us, apart from its first step. 'They straightforwardly adopt the three oldest *tonoi*, called Dorian, Phrygian and Lydian by derivation from the names of the races from which they originated – or for whatever reasons anyone else wants to think up – and assume that these differ from one another by a tone (perhaps that is why they are called *tonoi*)' (62.18–22). I shall not pursue the historical credentials of this remark (still less its attitude to speculations about the derivation of the names). I quote it only because

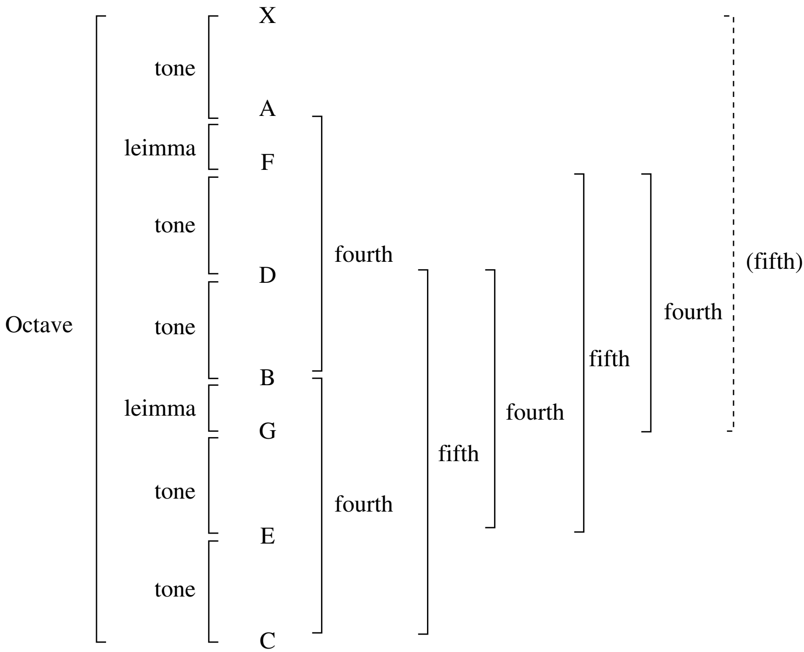


Fig. 9.08

it is here that the procedure attributed to these theorists differs most radically from Ptolemy's. It is the independent postulation of these steps of a tone that provokes his comment that 'one should not construct the concords out of the melodics'. As he also points out (62.22–3, and more explicitly at 56.4–7), no concordant relation has been produced at this stage of the other theorists' procedure, and it must therefore next set off in some quite different way to incorporate the three initial *tonoi* into a concordant framework. This is altogether too haphazard a beginning.

Ptolemy's own procedure is based squarely on the 'method of concordance' mentioned on p. 184 above. In principle it involves only a series of steps through the interval of a fourth downwards. This would obviously take us out of the octave range within which the *mesai* of all the *tonoi* are to be located; but since a step upwards through a fifth reaches the note an octave above the one reached by a step downwards through a fourth (62.2–9, 63.18–20), and since notes an octave apart are identical in function (58.21–59.2, cf. 13.3–11), upwards steps of a fifth can be substituted for downwards steps of a fourth whenever necessary.

Thus we start from some note, for instance *mesē*, designated by its function, in a first *tonos*, *tonos* A. To find the equivalent note in *tonos* B we move down through the interval of a fourth, and repeat the process to find that in *tonos* C. To stay within the octave we next move up a fifth (*tonos* D), then down a fourth (*tonos* E), then up another fifth (*tonos* F), and finally down another fourth (*tonos* G) (63.14–25). The procedure is shown in Figure 9.08, which also shows the spacings between *tonoi* that emerge from it (Ptolemy works them out at 63.25–64.10). The additional point marked X in Figure 9.08 is not of course the *mesē* of an eighth *tonos*, since it is an octave above that of *tonos* C and is functionally identical with it. It simply indicates the boundary of the octave in which the seven *mesai* are contained, and shows the size of its seventh interval (see p. 179 above).

At the end of II.10 Ptolemy gives the names traditionally attached to the *tonoi*, running in this order from highest to lowest. A is Mixolydian, F Lydian, D Phrygian, B Dorian, G Hypolydian, E Hypophrygian and C Hypodorian (64.11–13). He concludes the chapter with a sentence I quoted earlier. 'Thus the differences between them, which have been somehow or other handed down, have now been discovered by reason' (64.14–15).

One further point needs to be clarified. It is essentially straightforward, but confusions can easily creep in. Figure 9.08 shows the spacings between the functional *mesai* of the *tonoi*. But its sequence of intervals is not to be mistaken for the intervals in which any *systēma*, set in some genus and in any given *tonos*, will run its course. Consider two such *systēmata* in the tonic diatonic genus, whose tetrachords, starting from the lowest interval, have the form 28:27, 8:7, 9:8. Let one of them be Dorian, with its *mesē* at B, the other Hypolydian, with its *mesē* at G. From G to B is a *leimma*. But the interval above the note that is *mesē* by function is not a *leimma* in any *systēma*; it is the interval separating *mesē* from *paramesē*, which is always a tone. Hence the progression in the Hypolydian *tonos* upwards from G will begin with a tone (9:8), and go on into the tetrachord *diezeugmenōn* with steps of 28:27, 8:7, 9:8, entirely different steps from those separating the *mesai* of the higher *tonoi*. Similarly, while the first interval above Dorian *mesē* is again a tone, and its *paramesē* happens to coincide with note D, the progression is quite different from that point on. The steps between the *mesai* of the *tonoi* are not, then, except by coincidence, the steps of any *systēma*. Each *systēma* allocated to the pitch-range of a *tonos*, by the alignment of its *mesē* with one of the points indicated, will run its course according to its own genus and the set sequence of tetrachords and tones.

With these points in mind we can understand the passage at the beginning of II.11, where Ptolemy links each of the *tonoi* with one of the forms

of the octave. We are to imagine that the octave from C to X is the central octave of the double-octave sequence of 'positions'. In the terms of Figure 9.07, C is note 5 and X is note 12 (in Ptolemy's terms they are, 'by position', *hypatē mesōn* and *nētē diezeugmenōn* respectively). The forms of the octave, it will be recalled, are named as first, second and so on, depending on whether the disjunctive tone is highest, second-highest or whatever it may be in the octave's sequence of intervals. Since the interval above dynamic *mesē* is always such a tone, and since the other disjunction in the two-octave sequence will not appear in the octave between positions 5 and 12, the series of forms that this octave takes will simply follow the order of *tonoi* from the highest down. When the *tonos* is Mixolydian, with its *mesē* on A, the octave between C and X will have the disjunctive tone at the top, making the first form. When it is Lydian, with its *mesē* on F, the tone is second from the top, and the octave will take the second form; and so on for the rest (64.16–65.15).

When *tonoi* are conceived in this way, a modulation from one *tonos* to another does not involve a move to a different range of pitch, but one to a different disposition of the intervals within the same range.[11] It is on this basis, in the remainder of II.11, that Ptolemy attacks the views of certain theorists (identified in other sources as Aristoxenian) who pack more than seven *tonoi* into the octave, and specifically those who regard successive *tonoi* as lying a half-tone apart. From Ptolemy's point of view their construction makes no sense. The note that is *mesē* by function in the Hypodorian *tonos*, for instance, is located on note 5 (*hypatē mesōn* by position), and that of the Hypophrygian on note 6 (*parhypatē mesōn* by position). Then 'the *tonos* constructed between these, which they call "Lower Hypophrygian" in contrast with the other, higher one, must have its [functional] *mesē* corresponding either to *hypatē* [by position, i.e. note 5], like Hypodorian, or to *parhypatē* [by position, note 6], like the higher Hypophrygian' (65.24–30). That is, all the functionally designated notes in 'Lower Hypophrygian' must be in the same places in the series of positions as they are in one of the other two. In that case, when we move to the interpolated *tonos* from the one that is identical with it in that respect,

[11] In fact the pitch-range cannot be held exactly constant. Functional *mesē* in a *systēma* in the Lydian *tonos*, for example, is a tone plus a *leimma* below the top of the theoretically static octave framework. Two intervals must be fitted into this space. The first above *mesē* is a tone, but the size or ratio of the second will depend on the genus in which the *systēma* is set, and its upper boundary would coincide precisely with the top of the theoretical octave only if this interval were a *leimma*. In most cases it is not (it is so only in the eccentric 'ditonic diatonic'). Hence in some *tonoi*, attuned in some genera, the whole octave must shift up or down a little in pitch in relation to the static framework in which the *mesai* of the *tonoi* are placed. Ptolemy does not allude to this complication here, but it is reflected in the tables of II.15.

there will be no change at all in the form of the octave between note 5 and note 12. All that will have happened is that the *systēma* will have moved up or down by a 'half-tone' in pitch (65.30–66.1). 'Thus the *tonos* will not even seem to be different in form from the previous one, but will be the same again, Hypodorian or Hypophrygian, merely sung at a higher or a lower pitch' (66.1–3). Here Ptolemy draws a line under his discussion. 'Let that, then, complete our exposition of a rational and adequate account of the seven *tonoi*' (66.3–4).

Our own exposition has been long and complex, but it was essential if we were to form any considered view of Ptolemy's claim for the 'rationality and adequacy' of his account. We should conclude, I suggest, that in important respects it justifies his own assessment. Most of its claims have been argued out on the basis of the tightly knit group of principles and definitions discussed in II.3–5; and the details about the compass inhabited by the *tonoi*, their number and their spacings have all been extracted, as Ptolemy said they should be (57.28–9), from applications of a common axiom. This axiom, moreover, that homophones are prior to concords and concords to melodics, is one that was fundamental to the reasoning of Book I, and to the focus of the preliminary definitions in II.3–5. The whole construction, then, is worked out in such a way as to cohere both internally and with principles adopted and used previously, and it is astonishingly economical in the number of principles and assumptions that it employs. One of Ptolemy's aims was to show that in the domain he is investigating, 'nothing is produced . . . at random or just anyhow' (5.21–2). If that means that he was trying to exhibit the whole complex of harmonic structures not just as a collection but as an intelligibly integrated system of structures whose characteristics and whose relations to one another are determined by shared formal principles, where these principles themselves are also closely interlinked, his achievement is indisputably impressive.

I remarked earlier in this chapter, however, that he seems much less concerned, in this part of Book II, to show how the perceptible attributes of harmonic relations can be conceived as reflections of properties of corresponding formal relations. I identified something that might be construed as a minor example of such an explanation (p. 174 above). We can now add another on a larger scale, since we have been offered a clear, formal interpretation of an important group of musical intuitions about the relations between *tonoi* (pp. 184–5 above). But in general the point still stands. Nothing has been done to identify the perceptible, musical characteristics peculiar to each *tonos* or each form of the octave, and to analyse the ways in which they reflect properties of their formal counterparts. Even in the case of modulations between *tonoi* we have an interpretation

only of one, rather generalised fact – the fact that modulations between *tonoi* standing in concordant relations with one another are the more acceptable. No account has been given of the special character of each form of modulation, or of the way it is explained by the formal patterns underlying it.

The most substantial criticism that could be directed against Ptolemy's account of the *tonoi* is of a different sort; here I am picking up a point at which I gestured above (pp. 167–8). Given the way in which he defines his concepts and objectives, it is hard to find fault with his argumentation. Everything follows consistently and compellingly. But it may be that the very concepts that provided his starting point have prejudiced his conclusions in unwarranted ways. This suspicion is fuelled by the nature of Ptolemy's criticisms of rival theories. His view is not, in central cases, that they fall down over points of detail, or accommodate certain facts while failing to cater for others; he simply cannot make sense of them at all. It is unlikely that their proponents were offering suggestions merely at random. Nor can we reasonably suppose that the theorists who posited thirteen *tonoi* spaced at intervals of a half-tone, for example, were so unintelligent as to believe that their system would be well suited to the tasks that Ptolemy sets it. It is much more probable that their conceptions of *tonos*, and of the nature and role of modulation between *tonoi*, were different from his. It would thus have been open to them to applaud the cogency of Ptolemy's reasoning, but to reject the premisses from which it begins.

We can be reasonably confident that the notion of *tonos* to which the thirteen (later fifteen) *tonoi* related was a good deal closer than Ptolemy's to that of 'key'. I have speculated elsewhere about the ways in which these two notions, and the musical practices related to them, became intertwined and confused with one another,[12] and I shall not pursue that theme again here. Even in Ptolemy's account, as we have seen, the concept of key is not far below the surface. But he refuses to recognise it as one that is significant in harmonics. From his point of view, two *systēmata* that differ only in pitch do not differ relevantly at all.

Yet our evidence suggests quite strongly that musical practices in this period did indeed draw on the resources of key-relations, and the fact is reflected in the writings of other theorists (though often the two conceptions are confused). A good many instances of apparent key-change could of course be analysed in Ptolemy's terms, as projections of different interval-sequences onto the same range. In the context of an octave containing just seven melodic functions, however, this approach has its limitations; as

[12] See *GMW2* pp. 17–27.

he says himself in a passage we looked at above, in some cases a change of 'key' will introduce no change of *tonos*, in his sense, at all. But our own intuitions, at least, would suggest that a modulation of key through the interval of a semitone is not only significant but striking, and that the (more plausible) modulation upwards through a fifth from Ptolemy's Hypolydian, to a pitch at which no *tonos* can stand in his system, should be perfectly acceptable, and perceptibly different, for example, from a modulation to his adjacent Mixolydian.

Ptolemy's resistance to the view that 'mere' difference of pitch can be musically significant is presumably due to the way he understands an assumption embedded firmly in the foundations of his approach to harmonics. It is the assumption that differences perceptible and meaningful to the musical ear are reflections of differences in form. This is a guiding principle of his entire enterprise, and it surfaces in the present context in his determination to correlate differences between *systēmata* with differences in the forms of their constituent concords (II.3–4). Once that move is made, differences between the pitches of *systēmata* cannot as such be counted as real differences, and considerations of key cannot come into play. They have in effect been defined out of existence, or at least out of musical relevance. Yet Ptolemy had in his hands a strategy that could have been developed to accommodate them. The series of intervals separating one Ptolemaic *tonos* from another is not itself a *systēma*, as we have seen. Ptolemy nevertheless insists on regarding it as formed on rational principles, and on treating the shifts of pitch that the functional notes undergo in modulations as harmonically better or worse. Hence formal principles govern the structures not only of *systēmata* themselves, but also of the series of intervals between them; and the character of the series is musically significant. It seems clear that a similar approach could have been used to make sense of the relations between identical *systēmata* in different keys, and to attribute to them, too, aesthetically distinguishable roles. Significance would be attributed, not to formal differences between the *systēmata* themselves, since there would be none, nor to their pitches as such, but to the relations in which their pitches stand within the structure that contains them. A set of keys, so conceived, could live without serious friction alongside a set of Ptolemaic *tonoi*, performing distinct musical tasks. That Ptolemy ignores this possibility is due, perhaps, only to a failure of imagination. But his continuing confidence in his conclusions is shown by his insistence on submitting them, as always, to the judgement of the ear. Issues concerning those tests and the instruments deployed in them will be considered in the next two chapters.

10 The instruments

When we set out to use our ears to assess harmonic relations,

> there is needed to help them, just as there is for the eyes, some rational criterion working through appropriate instruments, as the ruler is needed to deal with straightness, and the compasses for the circle and the measurement of its parts. For the ears, similarly, which with the eyes are most especially the servants of the theoretical and rational part of the soul, there is needed some method derived from reason, to deal with things which they are not naturally capable of judging accurately, a method against which they will not bear witness, but which they will agree is correct. The instrument of this kind of method is called the harmonic *kanōn*, a term adopted out of common usage, and from its straightening [*kanonizein*] those things in sense perception that are inadequate to reveal the truth. (5.3–15)

It is some measure of the importance Ptolemy attaches to the use of such instruments in harmonics that he devotes nearly six whole chapters, and substantial parts of two more, to descriptions of their design and discussions of their properties. Issues to do with the procedures by which propositions are to be submitted to perceptual tests by means of these instruments are examined, sometimes at length, in at least a dozen other passages. These simple facts suggest that very close attention to the instruments and their practical manipulation is an essential element in the proper conduct of this Ptolemaic science; and that points in turn to the conclusion that Ptolemy's proclaimed allegiance to experimental procedures, designed as a testing-ground for scientific hypotheses, should be taken perfectly seriously. But there are often good reasons for treating warily the suggestion that this or that Greek scientist conducted genuine experiments to confirm or refute his hypotheses. It will therefore be as well to consider Ptolemy's pronouncements cautiously and critically, if not with downright scepticism.

Writers on harmonics quite commonly describe ways in which the truth of their theoretical claims can be exhibited in perceptible form. In most cases, however, there is no question of their regarding their procedures as experiments, in anything like the modern sense. That is, they are

not concerned to contrive operations whose perceived results will be allowed to determine whether a proposition offered for evaluation is true or not. The propositions are typically regarded as proved already by argument, especially by theorists in the 'mathematical' tradition. Methods for presenting theoretical conclusions to perception through the use of the monochord, for instance, were most often designed merely to illustrate the way in which such truths are manifested in the perceptible world, and thus to make them accessible to human minds enslaved to the impressions of the senses.[1] As we have seen, Ptolemy advertises his rejection of this approach. Perceptible counterparts of theoretical constructions must be offered not merely as illustrations and aids to mundane imagination, but as tests. Propositions argued for on a rational basis, but which conflict with what is perceived when they are submitted to the judgement of the ear, must be rejected (see e.g. 6.1–5, 6.9–11, and especially 32.10–13, 68.1–8).

But his official line on this issue does not settle the matter. It is not unlikely that some other writers also conceived the procedures they describe in a quasi-experimental light. They might have been prepared to agree, in principle, that if the 'experiment' in question gave the wrong results, that fact might cast legitimate doubt on their theory. But they would accept this only because they assumed that the experiment would in fact yield the results their theory required; and in a number of cases it is abundantly clear that the procedures envisaged were never carried out. If they had been, some of the results would certainly have come out 'wrong'.[2] We may guess that in such cases it would have been the experiment, rather then the theory, that would have been redesigned or reinterpreted to explain away the recalcitrant phenomena. Theories do not seem commonly to have been put seriously at risk by experimental tests.

If Ptolemy's practice matched his methodological pronouncements, then, he differs quite remarkably from the majority of theorists in the tradition to which his work most nearly belongs. We cannot actually watch what he did in his musicological laboratory, if indeed he did anything at all. But we should ask whether the way he discusses the relevant proce-

[1] See for instance Theon Smyrn. 57.11ff. (reporting the work of Adrastus), 87.4ff. and 90.22ff. (reporting that of Thrasyllus). The closing passages of [Eucl.] *Sect. can.* (props. 19–20), similarly, are by no means presented as making provision for empirical testing of the treatise's conclusions. They show merely how those conclusions can be applied to the task of dividing the string of a monochord so as to permit the accurate construction of a diatonic system. The accuracy of the conclusions is assumed in the procedure, not tested by it.

[2] This is notoriously true of the 'experiments' attributed to Pythagoras in Nicomachus *Ench.* 6.

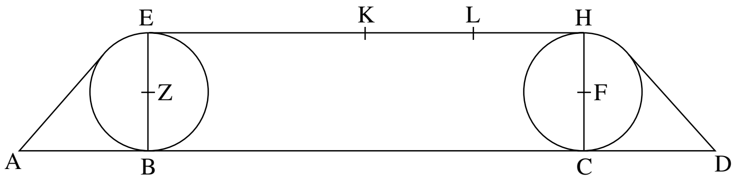
dures encourages the belief that he conducted such tests himself, or expected his readers to do so, and that he was genuinely prepared to reconsider his 'rational *hupotheseis*' in the light of experimental results. We must therefore review in some detail both his descriptions of the instruments and his accounts of the way in which the propositions at issue are to be represented, through their use, to the ears, as well as the nature of the results which he claims the 'experiments' will yield. In the present chapter we shall focus on the instruments.

In Chapter 3 we examined Ptolemy's discussion of the quantitative physical variables with whose values perceived pitches are correlated. The general upshot was that pitch depends on the degree of tension imposed by some agent on the air, and that this degree of tension varies, in turn, directly with quantitative alterations in various aspects of the agent by which the air is struck. The most relevant of these aspects is something straightforwardly measurable as a length, the length represented by Ptolemy as the distance from the striker (the agent) from the thing struck (the air). This is a characterisation we found hard to interpret in the central case, that of the plucked string. He believes himself to have established, however, that if the other relevant variables are held rigorously constant, changes in this length will be correlated in a very simple way with changes in the air's tension, the quantitative or formal aspect of the pitch of a sound. As the length diminishes, the tension imposed on the air increases; and the ratios of the lengths are the mirror-images of the ratios of the tensions, so that when the length is halved, for instance, the tension and the pitch are doubled.

His first discussion of the instruments, in 1.8, begins by sketching the difficulties involved in using some of the other varieties of sound-source that earlier writers on acoustics had appealed to. Ptolemy does not doubt that the pitches emitted by such devices have determinants of the sort he has indicated. But for present purposes we need an instrument on which the lengths, or other variables chosen, can be measured with maximal precision, and one in which we can ensure that the results are not distorted by uncontrolled changes in other properties that affect the pitch. Wind instruments are problematic for a number of reasons. It is hard to correct unevenness in the bore. It is also difficult to know just where to take the measurements of length – from the mouthpiece to an open finger-hole, certainly, but from which point, exactly, on the vibrating reed, and to which part of the hole? The relevant lengths, Ptolemy says, are 'established only approximately'. Finally, pitch is affected also by the 'blowings-in of breath'; and no means was available for ensuring that breath pressure and the pressure of the lips on the reed were held perfectly constant (16.32–17.7).

Some writers suggest, or attribute to the great Pythagoras, the thesis that the pitch-ratios can be displayed in the ratios between the weights of objects attached to suspended strings, so as to give them different degrees of tension.[3] This technique would rely on the hypothesis that where other factors are unchanged, the ratios between the pitches correspond directly to those between the tensions of the strings, as measured by the weights of the objects fixed to them. The hypothesis is one that Ptolemy would apparently accept in principle, though in fact it is false. He holds, nevertheless, that the method is unreliable. For one thing, it is difficult to find several strings that are identical in constitution, or for that matter even one that is perfectly consistent in all its properties throughout its length (17.7–12). These, however, are problems that Ptolemy himself must overcome, as we shall see shortly. More conclusively, he argues that even if these difficulties are resolved, and if the lengths of the strings are initially equal (as they must be to avoid interference by the separate pitch-determinant of length), they will not remain equal once the procedure is under way, since the weights will stretch the strings and increase their lengths, and the heavier weights will stretch them more than the lighter ones (17.12–16). And of course if we now adjust them again to equalise the lengths, we shall no longer have strings of identical constitution, since those that were stretched more forcefully will now be thinner. It seems fairly clear that Ptolemy did not realise that the results of such an experiment would fail to fit the theory, in the simple form that is presupposed, even if these difficulties could be resolved. (The ratios of the pitches would not be correlated with those of the weights, but with those of their square roots.) Questions in physical theory are not addressed here. Ptolemy is concerned only with the techniques by which an apparatus can satisfactorily be set up, given that the theories outlined in I.3 are sound. He is equally dissatisfied with 'demonstrations' of harmonic ratios that are contrived by percussion on 'spheres or discs of unequal weight, and with bowls, empty or full, since it is a very hard task to maintain identity of materials and shapes in all these things' (17.16–20). Again it is the practical problems that are uppermost in his mind, those of ensuring perfect consistency of material and of form, and of showing that such consistency has indeed been achieved when it has. This focus of attention evidently gives some preliminary support to the view that Ptolemy is envisaging instruments that are to be used and procedures that are to be carried out in practice; but equally clearly it is far from conclusive alone.

[3] E.g. Nicomachus *Ench.* 6, Theon Smyrn. 57.1ff. (attributions to Pythagoras), 59.4 (attribution to 'some people'), 65.10ff. (an argument of Adrastus).



The harmonic *kanōn*

Fig. 10.01

Fig. 10.02

If real operations on real instruments are to be conducted, to the standards of accuracy that these opening remarks seem to demand, the difficulties they raise are ones that Ptolemy must find ways of eliminating in the kinds of instrument that he favours. The measurement on which he will choose to rely is that of relative lengths of a stretched string, later of several strings. He has shown himself acutely aware of the need to hold constant all other variables affecting pitch, and it is essential for him to show that in the case of the stretched string, this can be done in practice. It will not be enough to talk glibly about an 'ideal string'; in theory, ideal pipes or metal discs would do just as well. The question is what could actually be achieved by techniques available to Ptolemy and his contemporaries. Ptolemy claims at once that the instruments he will use provide their own resources for resolving one of the two main problems infecting other sorts of device, and that they are immune to the second.

But the string stretched over what is called the *kanōn* will show us the ratios of the concords more accurately and readily. It does not acquire its pitch in any random manner, but in the first place it is equipped with a way of assessing any unevenness that might arise from the apparatus, and secondly its limits are appropriately placed so that the limits of the plucked sections between them, into which the length is divided, have suitable and clearly perceptible points of origin. (17.20–26)

These are very important claims. Ptolemy offers detailed arguments on behalf of their credentials, which will be examined below.

First, however, he provides a careful description of the basic structure of the one-stringed *kanōn* or monochord (17.27–18.9). The manuscripts give a diagram, of which a version appears in Figure 10.01. Like other diagrams in works of this period, it may or may not go back to the author himself, but in this case it fits the text well.

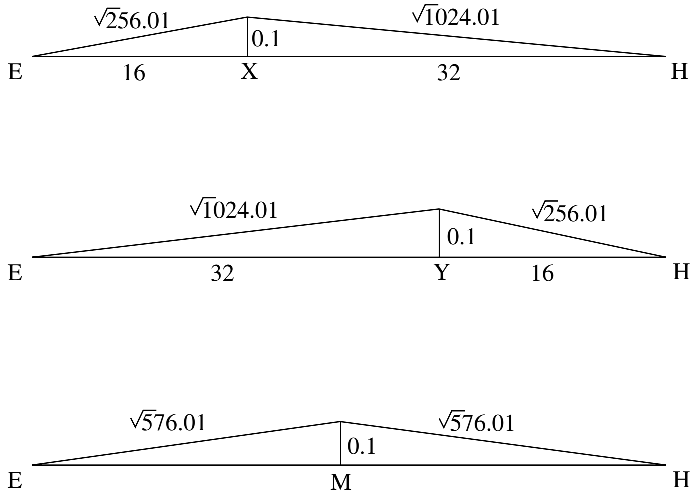
ABCD is the base of the instrument, and AEHD is the string. The two circles represent bridges determining the string's maximum sounding-length. In fact, as one would expect and as the text suggests, these bridges are not intended to be completely circular or spherical. The upper surface of each is a segment of a sphere, but the lower part will be either squared off, or formed as an upright cylinder with straight sides and a flat base, as in Figure 10.02.

These bridges, Ptolemy says, must be 'in all respects equal and similar, with the surfaces that lie under the strings spherical, as nearly as is possible. Let one bridge, BE, have Z as the centre of the surface mentioned, and let the other, CH, have F, similarly, as the centre, where points E and H are found by bisection of the convex surfaces. Let the bridges be so placed that the lines drawn through the points of bisection E and H and through the centres Z and F are perpendicular to ABCD' (17.28–18.4; see Figure 10.01). We shall return later to the question why the bridges should have this form, rather than being flat wooden slivers like those on modern instruments. But they do evidently have the advantage, from a mathematician's perspective, that the part of the string that sounds will meet the surface at a single, well defined point, since EH, if extended in both directions as a straight line, is a tangent to the circles EZB and HFC (see 18.4–9).

The next part of Ptolemy's account is designed to bear specifically on the resolution of one of the problems mentioned above. We shall consider that issue shortly; meanwhile the moves he makes can serve to indicate the nature of the remaining parts of the apparatus.

> To the string we shall now fit a measuring-rod [*kanonion*], and use it to divide the length EH, so that we may make the comparative measurements more easily. First, at the bisection of the whole length, K, and then at the bisection of the half, L, we shall place blades, very thin and smooth, or indeed other bridges, a little higher than the others but no different from them in respect of their placing, equality or similarity about a line through the middle of the convexity, which will be under the exact bisection of the measuring-rod or again under the bisection of the half. (18.9–17)

In most ways of using the instrument in its 'experimental' role, only one moveable bridge will in fact be needed; the second is introduced for a special, preliminary purpose. Ignoring this complication for a moment, we should notice Ptolemy's statement that the new bridge, or 'blade', must be a little higher than the others. From a practical point of view this is



Note that $\sqrt{256} = 16$, $\sqrt{576} = 24$, $\sqrt{1024} = 32$

Fig. 10.03

essential, if the string is to come into firm contact with the bridge. It is not a requirement that 'pure theory' would recognise, since from the point of view of mathematical geometry, line EH will touch a bridge at K perfectly satisfactorily if it is exactly the same height as the others. This again is some small encouragement to the view that Ptolemy was aiming at the construction of a real instrument, and one that would work in practice.

But it also introduces complications of at least two sorts. First and most obviously, EKH will no longer be a straight line, and it will therefore be longer than the original EH. The string will therefore be under greater tension. This would not matter if the new tension were the same wherever the moveable bridge is placed; but that is not so. A simple calculation will show that the extra length needed to form EKH will be least when the bridge is at the mid-point between E and H, greater when it is nearer one of the ends. Hence the ratio between the lengths plucked when the bridge is placed successively in two different positions will not correspond exactly to that between the pitches, since the latter will also be affected by the changed degree of tension imposed on the string. This is a problem that Ptolemy does not resolve or even explicitly mention. But the

omission is scarcely significant, since if the apparatus is constructed and used sensibly, the distortions involved can be made imperceptibly small. All that is necessary is that the extra height of the moveable bridge be very slight in relation to the length of the string, and that it never be placed too close to either end. Suppose, for example, that EH is 48 inches, and that the moveable bridge is one tenth of an inch higher than the others, which would be ample. Suppose also that instead of treating EH as the sounding length of the lowest note to be used, we make the sounding length XH, where X is the position of the moveable bridge 16 inches from E. This sounding length will be 32 inches, and to reach the note an octave higher we must move the bridge to Y, so that YH is 16 inches. None of Ptolemy's constructions require notes more than an octave apart to be sounded on a single string; hence the bridge need never be less than 16 inches from either end. It will be obvious that though the length and hence the tension of the string will alter as the bridge is moved to different points between X and Y, and will be least at the midpoint, M, the amount of variation is vanishingly small (see Figure 10.03). The overall length remains always within a whisker of 48 inches.

Further, since the string's tension is increased in these cases along with its overall length, and since greater tension raises pitch while greater length lowers it, the minute changes in these two variables will counteract one another, though they do not cancel out. As we shall see below (p. 203), it emerges in I.11 that Ptolemy had mistaken but not arbitrary grounds for believing that they will cancel exactly. It is quite likely that this is why he nowhere tackles the present issue explicitly, though statements in I.11 have a fairly direct bearing on it.

Moreover, one regular feature of his practice is well adapted, by accident or design, to minimising the errors that arise. One might have expected his measurements to be taken between points marked on the base of the instrument, at which the mid-points of the bases of the bridges would be located. Since the lengths of string bounded by the bridges are not absolutely identical to the lengths between these points on the base, very small distortions of the kinds we have been considering would be introduced. But Ptolemy always measures the string itself, by means of a separate measuring-rod (*kanonion*) which is held against it (e.g. 18.9–10). It is significant that points K and L in the sketch of the monochord (Figure 10.01) are said to lie 'under' (*hupo*) the relevant points on the *kanonion*, which they would not do if it were laid along the base. Ptolemy consistently pursues this procedure throughout his constructions; and though some minuscule error will still be involved, for the reasons we have given, it will be further reduced by these means. It is, in fact, genuinely negligible.

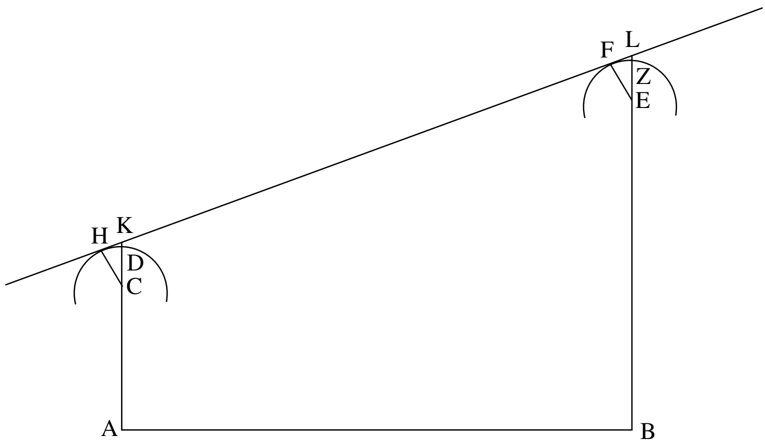


Fig. 10.04

The second difficulty is one that Ptolemy confronts head-on. If the moveable bridge is higher than the fixed ones, the point at which the string's sounding length meets each bridge will no longer be directly above the bridge's vertical axis; and the moveable bridge will not cut the string at a point, but will be in contact with it over a stretch of its surface's circumference. Ptolemy argues, much later in the work (III.2), that so long as the bridges' surfaces are parts of the circumferences of equal circles, the relevant measurements will not be affected. In the diagram attached to that passage (Figure 10.04), AD is a fixed bridge and BZ the higher, moveable bridge. Their upper surfaces are represented by segments of circles. K and L are the points above the axes of the bridges, and are those between which the measurements will naturally be made, and can be made with precision. But the string actually touches the bridges at H and F. (In fact, since after it has reached the moveable bridge, the string will come down at an angle on the other side, it will wrap over the top of the moveable bridge after meeting it at F. But this does not affect the matter.)

Ptolemy argues that if the tops of the two bridges are segments of equal circles, but not otherwise, the two triangles CHK and EFL will be similar and equal. Hence HK = FL, and HF, the sounding length, is equal to KL, the measured length. It is therefore of some importance that the bridges be made identical in this respect (89.33–90.5, 91.8–19). The case is set out in detail at 89.33–91.19, but we need not pursue it further; Ptolemy is evidently right. It is probably considerations of the sort set out here that lead him, in general, to prefer bridges of this sort to those of the blade-shaped type, since with the latter it is harder to be sure of the exact relation between the actual and theoretical points of contact between

bridge and string. We may reasonably be impressed, once again, by the amount of ingenuity he is prepared to devote to so small a practical detail of his apparatus.

Other minutiae about the placing of bridges will be considered later. For the present we shall return to 1.8. The instrument has been set up as in Figure 10.01, and points K and L marked as positions at which moveable bridges will be placed. Ptolemy does not immediately address the task for which the instrument is being set up, that of testing the perceptual credentials of the ratios assigned, on 'rational' grounds, to the concords. He still has to show that a further source of distortion in the results, analogous to one that affects the other sound-producing devices he has mentioned, can be identified and eliminated.

The relations between the pitches emitted by different lengths of the string will not correspond exactly to the ratios of the lengths unless the string is of even thickness and material constitution throughout. He proposes a way of checking a string's evenness in these respects. When a bridge is placed under K, and subsequently under L, 'if part EK of the string is found to be of equal pitch to KH, and again KL to LH, the string's evenness of constitution will be evident to us. If they are not so, let us transfer the test to another part, or another string, until the required consequence is preserved – that is, sameness of pitch in parts that are similar, corresponding, equal in length and of a single tension' (18.17–22). The suggestion that we 'transfer the test to another part' is elucidated later. Ptolemy is assuming that the string is substantially longer than the part of it that runs from one point of attachment, over the bridges, and down to the other (distance AEHD in Figure 10.01). One end of it is wound on a peg called a *kollabos*, which can be turned to alter its tension, just like a modern tuning-peg. At 81.5–9 Ptolemy suggests that a similar device would be helpful at the other end too (here *kollaboi* are referred to in the plural, since an instrument with several strings is under consideration). 'It will also be useful to attach additional *kollaboi*, equal in number to the others, at the opposite limit of the *kanōn*, to make it easy to shift the length of the strings along, when they are being tested, one of the *kollaboi* that hold them being relaxed, the other tightened.' It is a simple practical expedient to cope with a purely practical difficulty.

Returning to the sentence at 18.17–22, where the method of testing the string is described, it is obvious that the two sample tests that Ptolemy mentions would not by themselves be sufficient to guarantee that the whole length of the string is true. No finite number of such tests, in fact, would give a mathematically adequate proof. But the focus, here again, is on the best kind of assurance that is practically available, not on the theory of the continuum. The last part of the sentence seems to indicate Ptolemy's awareness that the two tests explicitly mentioned are not

enough. The implication is that further tests are to be made as often as we think necessary, over different samples of the sounding-length under scrutiny, until we are satisfied that the condition of sameness of pitch in parts of equal length has been met.

The procedure recommended here is not designed to establish the claim that equal lengths will give equal pitches if other factors affecting pitch are held constant. That proposition is assumed on the strength of the arguments in I.3. What is offered here is a way of discovering whether those other factors are indeed constant, in the particular piece of material that we have selected for use on our instrument. If equal lengths of it consistently give equal pitches, that is enough to show that it satisfies these conditions; and we can proceed to put the instrument to use. Ptolemy has thus argued that both the major defects that afflict other instruments can be avoided on the *kanōn*; we can accurately identify the points from which measurements should be taken, and with a little care we can ensure that no errors due to unevenness in the string's material constitution are allowed to creep in.

For all purposes beyond those of checking the ratios of the concords, in Ptolemy's view, an instrument with more than one string is essential (see especially 68.32–69.8). In most instruments of that sort, all the strings must initially be tuned in unison. They will then be divided by bridges to give different sounding lengths, at points determined by the series of ratios under consideration. It will thus become possible to play, in continuous sequence, all the notes of a postulated octave *systēma* or other such construction. Clearly each string must have properties equivalent to every other's. But to demand eight strings of exactly equal thickness and identical constitution would have pushed the resources of Ptolemy's workshop too far, nor would it have been possible to determine conclusively that this happy result had been achieved, if it had. The machine-made nylon strings or wires of modern times can indeed be treated for practical purposes as perfectly identical. The same was certainly not true of Ptolemy's twisted strands of gut; even with all the stratagems of twentieth-century technology, as every string-player knows, two gut strings from the same batch will never be exactly alike. Hence Ptolemy devotes the second half of I.11, in which an eight-stringed instrument is introduced, to a demonstration that the condition of precisely equal thickness and identical constitution does not need to be met.

What he sets out to show is that if equal lengths of the various strings give equal pitches, then the strings do not relevantly differ, even if some are thicker than others and are therefore subjected to higher tension in order to produce the initial unison (26.15–16). His argument is quite elaborate. He begins by setting out the propositions of physics on which it rests. There are three causes of pitch-variation in such agents; these are

density, thickness and length (see 7.17–9.15). In strings, 'tension is substituted for increased density, for it tenses and stiffens' (27.2–4). Hence 'if the other factors are the same, then as the greater tension is to the smaller, so is the sound based on the greater to that based on the smaller; and as the greater thickness is to the smaller, so is the sound based on the smaller to that based on the greater' (27.5–8). Thus when strings of equal length give equal pitches, but they are not equal in thickness and tension, this latter fact is of no consequence, since the variations in these two properties will invariably cancel out. 'And the ratio of the greater thickness to the lesser is always the same as that of the greater tension to the lesser' (27.8–13).

As we noted earlier (p. 51), this last proposition is false, in the sense in which Ptolemy probably intends it. Very possibly it was his confidence in the notion that where two factors affecting pitch are altered in opposite ways by the same change, their alterations always cancel out, that led him to think he could safely ignore the small concomitant variations in length and tension that we noticed on pp. 198–9. In the present case, fortunately, the falsity of his assumption would affect the issue only if it were tension or thickness, rather than length, that he now proposed to vary according to the required ratios, while holding other factors constant. We noticed also that it is most unlikely that he attempted to check by measurement the statements he makes here, since quite apart from inadequacies in his theory concerning tensions and thicknesses, his equipment for measuring them can hardly have been sufficiently accurate. The propositions, which he next presents *more geometrico*, in the form of theorems (27.14–28.12), are offspring of the theories of I.3, coupled with quite casual observation. Despite their suspect intellectual ancestry, for practical purposes the conclusions are correct. So long as the tensions and thicknesses stand in whatever relations are required to yield equal pitches in equal lengths, and are not subsequently tampered with, no errors will be introduced if we treat the strings as in all relevant respects identical. The potential rogue factors have been identified and held constant.

Ptolemy's description of the monochord is certainly adequate to give a craftsman the information he needs to build one and set it up for use. The simpler kinds of many-stringed instrument, which in the first instance are represented as having eight strings, are constructed, in effect, by straightforward repetitions of the same recipe. We have been told how to take our measurements, and reassured about some apparent problems relating to them; and we have been given instructions about the preliminary testing of the string or strings. When it comes to using them, the monochord, according to Ptolemy, has severe limitations, a number of which are discussed in II.12.

On the face of it, most of his complaints are beside the point. The first group, dealing with its alleged incapacity to demonstrate the harmonic

ratios accurately, refers only to its misuse by incompetent practitioners, not to defects in the instrument itself (66.24–32). The second group (66.32–67.20) points explicitly to weaknesses affecting it not as a device for scientific experiment, but as a performing instrument. (The division of the criticisms into these two groups has already been signalled at 66.15–23.)

If the length has been properly divided, it is possible, when ample time is taken in shifting the bridge, for the notes to be fairly well attuned to each other; but if its position is altered more quickly on account of the rhythmic continuity of the melody, this is no longer possible in the same way, since the appropriate marks themselves are not accurately located or precisely touched, because of the speed with which the shift in position is made. Indeed, so far as practical use is concerned, this instrument would be the last and feeblest. (66.32–67.4)

The awkwardness of the way it is manipulated, Ptolemy goes on, makes 'the finest products of manual technique' impossible on it, and he provides a list of such 'products' (67.4–10). The details have been variously interpreted; they seem to involve such things as the ornamentation of a melody, the playing of two notes simultaneously or as a trill, a musical legato, and rapid jumps upwards or downwards in pitch. It is also inevitable, we are told, that movements between pitches will involve 'most unmelodic' sorts of glissando (67.10–16). None of this seems relevant, at first sight, to the instrument's use as a device for testing the propositions of harmonic theory, any more than is Ptolemy's parting shot, that even its devotees never use it alone in performance, but accompany it with a wind instrument 'so that its errors may go undetected' (67.16–20).

I want to argue that Ptolemy's focus on the monochord's uselessness as a performing instrument is in fact both germane to the issues and significant for a correct understanding of his purposes. The whole series of criticisms was introduced with the remark that 'no substitute for it seems so far to have been devised, to provide, for the attunements worked out by reason for whole melodic sequences, a readily assessable form of comparison with what is perceived' (66.13–15). This suggests two thoughts. First, if we are to assess the credentials of a complete attunement worked out by reason, over the span of an octave, we must be able to play its notes in a continuous sequence, reasonably quickly and perhaps in several different orders, so that the ear may confidently compare their relations with those it expects and prefers. This is a point made again later (68.32–69.8), and it evidently identifies a requirement that will be difficult to meet on the monochord.

A second and more subtle point is hinted at in the expression 'for whole melodic sequences', literally 'in the melodies through wholes' (*en tais di' holōn melōidiais*). When a theorist has offered an analysis designed to capture, in mathematical form, the structure of the attunement underlying certain familiar kinds of melody, the correctness of the

analysis will not be reliably assessable, except, perhaps, by the ear of a very experienced instrumentalist, if we merely run through the notes of its 'scale'. It will be a great help if we can actually play some of the tunes themselves on our apparatus and listen to the way they sound. This seems to be what the expression means; and in that case some at least of Ptolemy's comments on the monochord's defects as a performing instrument can be put into an intelligible and relevant context.

That this is the right interpretation is indicated by some remarks in II.13, where Ptolemy considers attempts made by Didymus to find better techniques for using the monochord, and offers criticisms also of the generic divisions he postulates. Didymus 'failed to achieve what was necessary, however, in that he concentrated solely on making the bridge easier to manipulate, being unable to find a cure for the other more numerous and serious defects which we have described' (67.22–4). The idea behind Didymus' innovations is that when the bridge is placed two thirds of the way along the string, for instance, so that the pitch of its longer segment is a fifth higher than that of the whole string, the shorter part can also be used to sound a note, in this case one an octave above that given by the longer. Hence two notes can be formed with a single position of the bridge, and the same note can be made available by two different positions of the bridge, on one side of it when the bridge is in one position, on the other when it is in the other. Ptolemy agrees that this can be helpful, in a minor way (67.24–68.10).

> But it makes the method more difficult, when the melody [*to melos*] does not conjoin common notes [i.e. notes available while the bridge is in a single position], in that the different positions of the same notes raise the question which of them is to be used, since the continuous activity of plucking does not allow any time for thought; and by comparison with a choice between several possibilities, an approach through a succession moving always in one and the same direction would be more ready to hand. (68.10–15)

The general sense of this is clear and the point legitimate. The problems Ptolemy indicates would scarcely daunt any talented and well-practised exponent of Didymus' technique, but perhaps there were none; and the trick could certainly not be picked up easily by a mere musicologist. But Ptolemy seems again to be talking about the performance of actual melodies; and this angle of approach reappears a little later, after he has set out and criticised Didymus' divisions of the tetrachord. The reason for his errors, Ptolemy says, 'was his failure to embark on his postulation [*hupothesis*] of the ratios with sufficient circumspection, having neglected to consider in advance the way in which they are used in practice; only this makes it possible for them to be brought into conformity with the impressions of the senses' (68.32–69.1). Melodic ratios, unlike those of the concords, cannot be demonstrated on a single string. We need the full

eight, adjusted in the way that was described earlier, 'these being adequate to display to the hearing the sequence of the melody' (69.7–8).

In this last phrase the word translated 'sequence', *heirmos*, is not a technical term of harmonics, though it may have been current among musicians. If Ptolemy had meant anything like 'scale', he would almost certainly have used the word *systēma*; and his adoption of this much less formal term (related to a verb used commonly for such activities as stringing beads on a necklace) indicates a less formal sense. It refers to the 'course' through which we all hear the melody running, not to the 'scale' which experts may identify as its underlying structure. The strings, then, must be adequate to present to the ears the melodies themselves, those alleged to be grounded in the attunement under scrutiny. Didymus went wrong, similarly, through inattention to the way in which the pitch-relations are actually used. Where they are actually used is in tunes; and what he failed to do was to condition his *hupotheseis* in advance by consideration of these uses, and to try out his ratios in the context of use, 'in the melodies through wholes', whole compositions. The present passage, taken with the contents of II.12, can hardly be understood unless this is what Ptolemy means. His comments remind us that it is the attunements of real musical practice, not merely ones that sound acceptable as musical possibilities, that he is ultimately seeking to analyse and recreate.

More remarkably, they point towards his grasp of the fact that the ear will recognise faulty musical relations much more readily when they are embedded in the performance of familiar melodies than when they are presented only in the context of a formal framework such as a scale. This thesis is no sort of *a priori* truth. What underpins it is experience in the business of actually assessing such relations. Any experienced musician will agree that a set of relations that sounds acceptable as a scale may no longer do so when used to support a melody of the sort for which it was intended. But this is not a point that would be likely to concern a mere armchair theorist, or even to occur to him. It is important if, and only if, we are to take the notion of 'empirical testing' seriously. The fact that Ptolemy spends substantial parts of two chapters on issues that are relevant only in the context of a belief of this kind must count as further evidence of his good faith, and his practical experience, when he insists that attunements must be submitted to the judgement of the ear. No other Greek writer on mathematical harmonics, so far as I know, shows any sign of appreciating the need to present attunements for the critical ear to assess, not as bare structures or scales, but at work in the melodies whose foundations they are alleged to be.

Ptolemy allows that the monochord is adequate to let us assess the ratios that theory assigns to the concords. To deal with melodic relations an instrument with at least eight strings is essential. But a second sort of device, called the *helikōn*, we are told, 'has also been made by students of

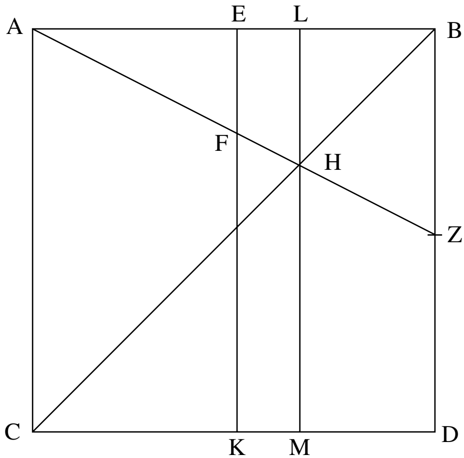


Fig. 10.05

mathematics to display the ratios of the concords' (46.5–7); and another sort of eight-stringed instrument can be developed by an extension of the principles that govern the *helikōn*'s construction. (It is possible that this latter device is Ptolemy's own invention.) Both instruments are described in II.2, with a few additional remarks in II.16. Ptolemy's main interest is in the more elaborate of these devices, rather than in the *helikōn* itself; and in II.2, III.1 and III.2 he offers comments on its merits and defects, comparing them point by point with those of the simpler instrument that is, in effect, merely eight or more monochords lined up on a common base.

The geometry of the *helikōn* and the ways in which the ratios of the concords are found on it are described in detail at 46.7–47.17, with the help of the diagram reproduced in Figure 10.05. ABCD is a square. AC, EK, LM and BD are strings, and AB and CD represent the positions of the fixed bridges. The diagonal BC is merely a line of construction, not corresponding to any material part of the instrument; but AZ is another bridge. Its function is comparable to that of the moveable bridges on the earlier instruments, but while they had a separate bridge for each string, this one is continuous, and apparently of the blade-shaped variety,[4] lying under all the strings in a straight line. String EK bisects AB and CD, and Z is at the midpoint of BD. The position of string LM is determined by that of point H, through which it passes; it will in fact be two thirds of the way from AC to BD. Notes can be sounded from the segments of the strings on either side of the bridge AZ. These segments will stand in the following relations.

[4] It is described at 81.11 as 'flat'; compare 18.13.

AC is twice BZ and ZD. EF is a quarter of AC, and FK three quarters. LH is one third of AC, and HM two thirds. Ptolemy suggests, by way of example, that AC be treated as 12 units long, FK as 9, HM as 8, ZD and BZ as 6, LH as 4 and EF as 3 (47.4–7). Then the ratios of the concords, and of the tone, will appear in the following relations: the fourth (4:3) in AC:FK, HM:ZD and LH:EF; the fifth (3:2) in AC:HM, FK:ZD and BZ:LH; the octave (2:1) in AC:ZD, HM:LH and BZ:EF; the octave and a fourth (8:3) in HM:EF; the octave and a fifth (3:1) in AC:LH; the double octave (4:1) in AC:EF; and finally the tone (9:8) in FK:HM (47.7–17).[5]

This ingenious construction has clear advantages over the monochord for the purposes of exhibiting and confirming the concordant ratios. No bridges need to be moved; the whole system of relations is presented at once, and any two notes can be played in as rapid a succession as one wishes, or even simultaneously. (This is a useful fact, since Greek writers typically define concordance in terms of the aesthetic effect produced when two pitches, in appropriate relations, are sounded at the same time.) But Ptolemy, as I said, is more interested in the possibility of extending the principles underlying its construction, so as to produce a similar device on which the ratios of melodic intervals spanning the complete octave can be produced, using whatever pattern of tetrachordal division one wishes to try out. The main feature of the *helikōn* that is carried over into the new instrument is that the sounding lengths of its strings are determined by the strings' lateral displacement from left to right across the face of the apparatus. The fact that this is true of the *helikōn* can easily be shown. Suppose that line CD is extended to the right, to a point Q, so that CD = DQ. It will then be seen that the ratio between the distances along this line, from Q to the ends of any two strings, is identical with the ratio between the sounding lengths of these strings, that is, the lengths running from their intersection with CD to the bridge AZ. Thus, for instance, QC:QK = 4:3 = CA:KF; QK:QM = 9:8 = KF:MH, and so on.

For his new instrument, Ptolemy draws up a second diagram (see Figure 10.06) in which the square ABCD is replaced by a rectangle. (This change seems designed merely to indicate that the rectangle's proportions are of no consequence.) Strings at AC and BD, when the construction is complete, will give the outer notes of an octave, as before. Then, Ptolemy says, 'we add DE, equal to and extending CD, and cut the side CD, by the application of measuring-rods [*kanonia*], in the ratios proper to the genera, making E the limit of high pitch' (48.2–4). This slightly opaque remark is to be understood in the sense indicated above. The strings are to get progressively shorter and hence higher in pitch as they approach E, until at E,

[5] A briefer and sketchier account of the instrument is at Aristides Quintilianus 98.2–99.12.

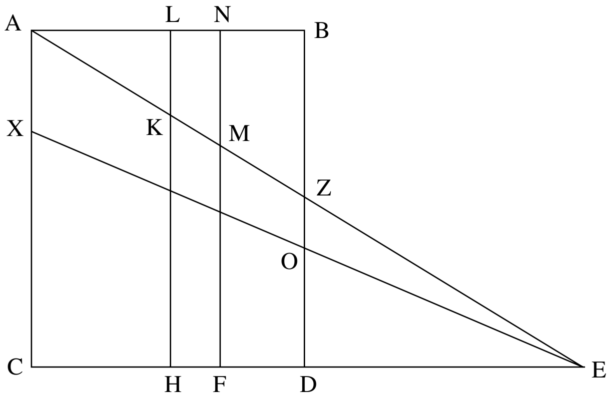


Fig. 10.06

the 'limit', no length would be left. The ratios in which the line is to be divided are again those of distances measured from E, as Ptolemy makes explicit a few lines later (48.10–14). When he says that we 'cut the side CD . . . in the ratios proper to the genera', he does not mean that the measurements are to be taken from D, which would be nonsensical, but only that all the points of division, the boundaries of the relevant distances, fall between C and D, the points at which the strings bounding the octave are located.

Next,

> through the resulting points of division on CD we stretch strings parallel to AC and equal to one another in pitch; and when this has been done we place under them what will be the bridge common to the strings in the position, AZE, that joins the points A and E. In this way we shall make all the lengths of the strings in the same ratios [as those of the lengths between their ends and E], so that it makes possible the assessment of the ratios that have been assigned to the genera. For as the distances taken from E along CD stand to one another, so will the strings taken upwards from their limits, parallel to AC and as far as AZ, stand to one another; for instance, as is EC to ED, so is CA to DZ. Hence these strings will make the octave, since their ratio is 2:1. (48.4–14)[6]

Ptolemy's description is clear, and the construction has the properties he claims for it. The instrument would work, and we need not argue the

[6] In the Greek there are no nouns corresponding to the word 'distances' and to the two occurrences of 'strings' in the last two sentences. There is only in each case the feminine plural form of the definite articles, *hai*, meaning 'those'. The only feminine plural noun in the vicinity is 'strings', which appears several times in the preceding clauses, and must be what *hai* refers to. But 'strings' is inappropriate in the first place where *hai* occurs, since there are no strings from E along CD. Ptolemy has been a little careless in his expression. My substitution of 'distances' for 'strings' here is, I hope, a harmless correction.

point. But it is important to take note of the order in which the stages of his account are arranged, and to compare that order with the one adopted in the account of the *helikōn*. In the description of the *helikōn*, the first steps mentioned are the construction of the square ABCD, the bisection of AB at E and of BD at Z, and the construction of AZ and BHC. EK and LM are added next (46.7–10). This seems the natural way to proceed if one's mind is on the geometry of the figure. Pure geometry is pursued, in fact, right through to 47.1; and only then, starting at 47.2, is an interpretation offered in terms of strings and bridges.

In the case of the second instrument, by contrast, as soon as the initial rectangle has been drawn up (47.18–19), we are told that AB and CD are the end-points of the strings, and that AC and BD are the boundaries of the octave (47.19–48.2). Next CD is produced to E, and CD marked at the points corresponding to the ratios of lengths, 'making E the limit of high pitch' (48.2–4). Then we place the strings and equalise their pitches (48.4–6); and only then, 'when this has been done [*toutou genomenou*]', do we insert the bridge AZE (48.6–8). From the geometrical point of view this is inelegant, if not downright confusing. We are required to introduce minor and variable details of the figure (the points marking the distances along CE, and the lines joining them with AB) before its main structure is completed (AZE still remains to be drawn); and musical and material details are included along with geometrical ones, instead of being left for a later, interpretative stage. In particular, Ptolemy does not say, for instance, 'we draw lines parallel to AC, and a line joining AZE'. He says 'we stretch strings parallel to AC . . . and . . . place under them what will be the bridge . . . AZE'.

But from the perspective of someone who is actually setting up such an instrument for use, step by step, this is the right order. We need to know the practical function of each element in the figure as we proceed, in order to understand what it is, physically speaking, that we are required to do. Most significantly, it is essential to fix the strings in place before adding the bridge, since they can all be adjusted to the same initial pitch only at a stage before the bridge is inserted. Thus while the account of the *helikōn* reads like a passage from a treatise in geometry, subsequently given a concrete application,[7] the second account is more like a set of instructions from a 'Build-your-own-instrument' manual; and I see no reason to reject the implication of this mode of approach. Ptolemy intended that the instrument should really be made.

The closing lines of II.2 offer comments on the advantages and disadvantages of this device. They are compared with those of 'the first method' (48.23–4), which is not a reference to the *helikōn*,[8] but to the

[7] We may note that Ptolemy himself attributes it to 'mathematicians' at 46.6.
[8] As I misleadingly suggested at *GMW2* p. 321 n.23.

eight-stringed instrument developed directly from the monochord. That instrument, we are told, is easier to use than the present one in one respect: there is no need to move the strings themselves sideways into new positions in order to create a new attunement, or to have strings spaced at different distances (48.23–5, 49.1–3). The new instrument is at an advantage in that there is only one bridge; we do not have to manipulate a whole set of little bridges (*hypagōgidia*) in order to accommodate a different pattern of tuning (48.25–6, 48.30–49.1). Further, it is possible on the new apparatus 'to move the bridge down, pivoting on E, to the position of XOE, so as to make the whole pitch higher, while the special character of the genus remains unchanged. For as CA, for instance, is to ZOD, so is XC to OD, and similarly for the others' (48.26–30).

This is clearly true, since the construction was independent of the proportions of ABCD; and X can lie in any position we choose between A and C. Ptolemy does not explain here why this possibility is any sort of advantage, though an application of it is implied in III.2 (see p. 217 below). Several other reasons could be imagined. It would enable us to bring the attunement into a range where its intervals were more easily evaluated by ear. We could adjust one such instrument to a range an octave above another, so allowing a complete double octave to be represented. We could align its pitch with that of a regular performer's instrument, to check the similarity of their attunements, or we might accommodate it to the range in which a melody could conveniently be sung, to test the instrument's melodic relations against those of the singer. In any of these cases, the adjustment is significant only in the context of the instrument's practical use. It adds nothing to its theoretical virtues.

Ptolemy has remarked that the need to shift the strings sideways to adjust an attunement or to construct another one creates a problem for this instrument, again a problem of a purely practical kind. The difficulty may well seem acute, in fact; on stringed instruments of the usual sort, the business of shifting strings sideways along the instrument between precisely determined points would be fiddly and awkward in the extreme. It appears that he subsequently gave the matter some thought, for he returns to it with a solution at the end of II.16. The passage is at 81.9–21, immediately after the sentence recommending the attachment of a second set of *kollaboi* (see p. 201 above).

It will also be useful to make them [the *kollaboi*] moveable on their *pelekēseis*, over the breadth of the *kanōn*, for the sake of a second form of usage, in which a single flat bridge is placed under the strings, and the sideways movements of the strings make the appropriate attunements. For when two *kanones*, equal to the length of the fixed bridges, are divided once again into the parts lying between the

outermost notes, and when one of the two *kanones* is placed against each of the two bridges, in such a way that equal numbers are placed opposite one another against the same points, the sideways movements of the strings will be displayed by these numbers, for people who are capable of making an attunement. If the *kollaboi* themselves go along with them too, the notes will retain the same pitches, but if the *kollaboi* stay still, the result will be that the strings, being sometimes slackened and sometimes tightened as a consequence of the sideways movement, will once again need to be restored to their original equality of pitch.

This is as pretty an example as one could wish for of Ptolemy's attention to the fine details of his devices, and to the minor pieces of gadgetry needed to make them work well, in cases where no theoretical issues are at stake. The *kollaboi* or tuning pegs will not be set directly into the base of the instrument. Each is inserted into its own *pelekēsis*, a small piece of wood whose name suggests that it was probably wedge-shaped.[9] A hole is drilled through each *pelekēsis*, so that it can be fitted, along with the others, onto a rigid bar that runs parallel to, and a little outside, the line of the fixed bridge, CD. An identical device is located at the opposite end of the instrument, to receive the other end of the string; the corresponding bar runs along the far side of AB. The string can then easily be moved sideways, as the *pelekēseis* at each end of it are slid along their bars. The two *kanones* or measuring-rods, identically calibrated, are placed up against the length of the two fixed bridges, AB and CD, or perhaps attached to them (*prostithemenōn*), so that by making the string run across equivalent points on the *kanones* we can ensure that it remains parallel to AC and to the other strings.

Further questions about the uses of the many-bridged and the one-bridged instruments will be raised in the remaining passages we have to consider, III.1 and III.2, but they cannot be discussed until we have set them in their context. Here, though Ptolemy is looking at new issues to do with the design and use of his instruments, they are still issues that centre around the problems of practice.

Both chapters are concerned with the project of representing the whole gamut of fifteen notes on the strings of an instrument, though it is a project that Ptolemy thinks is strictly unnecessary in harmonic science; a single octave would in principle be sufficient (83.1–9). The obvious strategy is to use fifteen strings. In every kind of device that has so far been described, all the strings must be the same length, and must be tuned, prior to the insertion of moveable bridges, to the same pitch. The most

9 The word is related to the noun *pelekus*, 'axe', and might be construed as referring to something shaped like an axe-head. The uses of various other cognate terms suggest, however, that it means 'wood-chip', and indicates a piece of wood shaped like those characteristically produced by the blows of an axe.

pressing problem, then, is that as the strings are shortened to raise their pitch, they may come to lose sonority (becoming *dusēchous*, 'poor-sounding'); and it is also a finicky and tedious task to have to work out, and mark on the measuring-rod, all the divisions of its length that are needed to complete the double octave (83.9–12).

Ptolemy suggests a simple way of resolving both difficulties at once. Of the fifteen strings, eight should be quite fine, and tuned initially to the same, fairly high pitch, while the remaining seven should be thicker, and are tuned an octave below. Thus if we number from the bottom, strings 1 and 8 will stand an octave apart, and will remain so. Strings 2 to 7 will be divided by bridges at the same distances from the origin as strings 9 to 14 respectively, and the bridge under string 15 will of course divide it in half, so as to make the octave with string 8. The two octaves will accordingly be divided in the same ratios. 'Thus a division of only one octave [on the measuring-rod] will be fitted to the two orderings [of strings], making the ratio of an octave between each of those that ought to be homophones' (83.19–21). Ptolemy follows this account with a formal proof of the soundness of its procedure (83.22–84.10). It is based on the principle that if there are two strings of equal length, AB and CD, giving different pitches, and if AB is divided by a bridge at E and CD at F, where AE and CF are equal, then the relation between the pitches of AE and CF will be the same as the relation between the pitches of AB and CD. The argument is reminiscent of the one given in I.11 to show that strings with equal pitches in equal lengths can be treated as identical, irrespective of their tensions and thicknesses (27.14–28.12; see pp. 202–3 above); but unlike that one, there is nothing suspect about the present argument's premisses. We need not discuss it further.

The remainder of III.1 considers three points. Each is sensibly addressed, but together they are a somewhat miscellaneous bunch, not closely integrated into a single line of thought in Ptolemy's usual manner. He argues, first, that the kinds of instrument he has been discussing can be used in either of two ways, by people with different competences. A person who has no musical ability beyond the capacity to recognise unisons can nevertheless set up an attunement correctly (84.11–12). This is obvious, since once the strings have been tuned in unison, the bridges are located on the basis of mathematical calculations, or tables recording their results, not by ear. The point could only be relevant to the business of harmonics, however, if the ability lacked by the hypothetical person was just that of forming a correct attunement by ear. The procedure would be of no use to him if he were too tone-deaf to recognise correct and incorrect attunements when he heard them. It might be only the former incapacity that Ptolemy means to indicate, relying on his

thesis that ‘judging is in general easier than doing the same thing’ (4.7–8). But if he means that, he has not said it; the person is said to be able to ‘grasp’ (*antilambanesthai*) only unisons, not to be capable of ‘constructing’ nothing else. In that case Ptolemy’s statement seems true but pointless.

But it is only a passing, one-line remark, designed mainly to introduce the contrasting case. This refers to a person who can set up an attunement correctly by ear, and what Ptolemy explains is that he can proceed by a route that reverses the manoeuvres that have so far been employed. He can begin with the strings tuned just anyhow, place the bridges in their mathematically computed positions, and then adjust the strings’ tensions to produce what his hearing accepts as the relations proper to the corresponding attunement. If his ear is reliable, and if the postulated ratios are correct, it will turn out that when the bridges are moved to the positions mathematically appropriate to a different attunement, the pitch-relations will still be perceived as true, since without doing so deliberately, the student must have adjusted the tensions so that equal lengths of the strings sound equal pitches. Ptolemy argues all this out quite fully (84.12–85.8). The procedure he describes has affinities with one used in II.1, and we shall return to it when we review that passage in Chapter 11. For the present the details are not important. At a general level, however, Ptolemy’s discussion serves to illustrate his readiness to consider his testing procedures from several angles, and to find different practical approaches to the same end.

The second point in this series is a broad reassurance, and a reminder. We should not be worried by the fact that it is no longer one single string that is being divided in the appropriate ratios. The ones we use are

> potentially . . . no different from a single string . . . For the task we assigned to the *kanōn* was not that of displaying the ratios of the melodics through a string that is numerically one, or through a plurality of strings whose number is determined, but that of using any number whatever of strings of equal pitch, such that they present themselves as no different from a single string, to make by reason alone that same attunement which the most musical people would make by ear. For the sake, most importantly, of exhibiting the quite incomparable skill with which the works of nature are crafted, and for the sake, secondly and in consequence, of promoting the practice that makes use of it, it is essential that this sort of method be adopted as a foundation, for the discovery and the exhibition of the ratios that make attunement accurate. (85.11–19)

The closing, inspirational remarks are a clear reminiscence of I.2, where the general aims of harmonic science were set out. The meaning of the preceding sentence is clear enough, but it is harder to guess at the nature of the doubts of those to whom it is addressed. Why should anyone

suppose that harmonic divisions can properly be represented only on a string that is 'numerically one'? Perhaps the disposition to think in this way was simply a reflection of the honourable place held, in the tradition of mathematical harmonics, by the ritual of 'dividing the string' on a monochord. Or perhaps Ptolemy is alluding to people with more metaphysical motives and assumptions, who saw harmonic division as a counterpart of the original differentiation of a cosmic unity into an ordered plurality. This would be a conception of an authentically Pythagorean sort. Whichever constituency he has in mind, Ptolemy is arguing that the practical requirements of a procedure 'for exhibiting the quite incomparable skill with which the works of nature are crafted' must take precedence over the theoretical purity of the division of a single string; and that in any case, even from a theoretical point of view, the results of the two procedures are equivalent.

The final paragraph of III.1 returns us to the two different kinds of many-stringed instrument, one with a moveable bridge for each string, the other the derivative of the *helikōn*. In the present context, Ptolemy says that he has 'no fault to find' with the former of these, 'so long as the whole *systēma* is divided up into two sets of similar divisions, in order that the differences that have been expounded may all be attuned' (85.19–23). He is thinking here of the two sets of strings, tuned initially an octave apart, that he discussed a little earlier; and these are still in his mind when he turns to the second instrument. One bridge will no longer be enough, since the same lengths are to be produced in each of the two sets of strings, but just two will be sufficient, one under each set. With this apparatus, however, 'it will often happen that the strings located by the ends of the bridges, in the middle span of the *kanōn*, come up against the ends of the bridges lying opposite to them, in the sideways movements involved in shifts of tuning, and so can no longer maintain their proper lengths. Hence it is possible by this latter method to determine only those *systēmata* in which one or other of the notes mentioned keeps the same position in the shifts of tuning; this happens particularly in those played on the *kithara*. Only in these *systēmata* is it sufficient to use these continuous bridges in the way described; and as a result the *kollaboi* of the fixed, common notes in these *systēmata* can stay still, without any sideways displacement' (85.24–34).

The difficulty Ptolemy identifies seems very slight. If the string giving the highest pitch in the lower set has to be moved sideways to raise its pitch, or if the string giving the lowest pitch in the higher set has to be moved to lower its pitch, in order to produce a particular pattern of tuning, then either may come in contact with the bridge belonging to the other set of strings. But since there are two quite independent bridges,

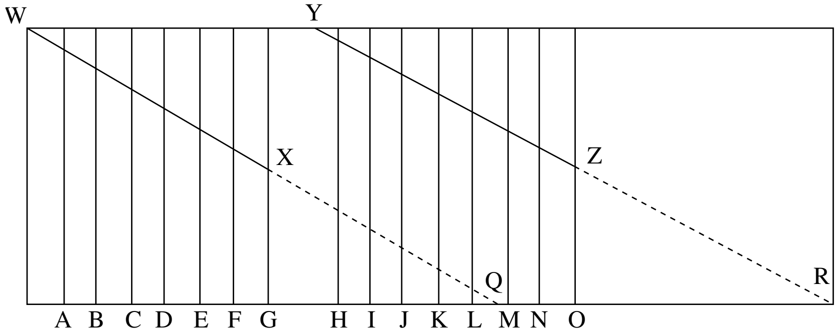


Fig. 10.07

there is no need for the two sets of strings to be at any particular distance from one another (see Figure 10.07). It would surely be possible to separate the two sets sufficiently to avoid the contingency which Ptolemy is concerned about, in any form of tuning; and this is a fact which he seems, most curiously, to overlook.

Perhaps I have misconstrued the problem, but I can find no better interpretation. I can account for Ptolemy's oversight only by one, quite unsubstantiated hypothesis, that he actually had an instrument of the kind in question, arranged to permit tuning over two octaves with two bridges, and that it happened to be constructed in a way that took no account of the need for extra spacing between the sets of strings. Ptolemy's failure to think of an appropriate modification is still uncharacteristic. But his remarks about the *kithara* tunings (those discussed in I.16–II.1, II.16) are correct, or nearly so. Both the strings in the middle keep the same pitches, in relation to the extreme notes of the octaves, in all these attunements, except that in two of them one of the strings must shift. In one case the movement is very slight, but the other is substantial and might lead to the difficulty Ptolemy identifies.[10] At all events, the problem is plainly of a practical order, and would trouble no one who was not concerned with real instruments, and with every aspect of their structure and manipulation that might put their accuracy at risk when they are used. This particular problem is so trivial and so easily remedied that Ptolemy seems to be worrying unnecessarily.

Much of III.2 is also concerned with ways of setting up an instrument to take an attunement over the complete double octave. This time Ptolemy suggests techniques for representing such an attunement on only

[10] For details see *GMW2* p. 365 n.10.

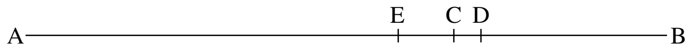


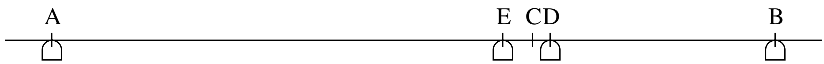
Fig. 10.08

eight strings. His exposition begins as follows. 'Take AB as the measuring rod fitted to the whole length, and divide it at point C, so as to make segment AC double CB. Then take, in each direction from C, first CD, in the direction towards B, and then CE,in the direction towards A, so that the whole of DE divides off the width of one of the moveable bridges, or a little more, while EC is double CD, so that the remainder AE is still double the remainder DB' (86.2–8). The construction is shown in Figure 10.08.

Here the 'whole length' is best understood as being that of a string between the fixed bridges, rather than that along the side of the instrument related to the *helikōn*. As Ptolemy explains later (89.15–25), the present technique can indeed be adapted for use on that instrument, but not in all the forms in which it will be described; and he will give reasons for preferring the form to which, as it happens, that instrument cannot be accommodated. It will in any case be simpler, at the outset, to think only of an instrument each of whose eight strings has its own separate moveable bridges.

This plural, 'bridges', is correct, for there will be two such bridges to each string. In its essentials the method is related to the one attributed to Didymus in II.13, in that it uses two segments of a string to produce different notes, one from each end of its span. If AB in Figure 10.08 is now taken to represent a string, rather than the rod by which it is measured, E and D are the points at which it crosses the two moveable bridges. That is, they are not points lying above the edges of the bridges, but above the centres of their convex surfaces. Ptolemy stipulated that DE is the width of one moveable bridge, not of two, and it will be occupied by half the width of each of them. The phrase 'or a little more' allows for a gap to be left between them, which will make them easier to move. Mathematically, they could touch. If a moveable bridge is 6 units wide, and if no extra space is allowed, so that EC is 4 and CD is 2, then when the bridges are placed with their centres below E and D, they will touch at a point 1 unit to the left of C. The space is for practical convenience only, as its engagingly vague description suggests. In theory, then, the situation is as in Figure 10.09, with the lengths given below it by way of example.

In fact, as Ptolemy explained when he introduced the basic monochord



AB = 360

AC = 240, CB = 120, so that AC = 2CB.

Width of each bridge = 6.

Least possible distance between E and D = 6, where EC = 4, CD = 2.

Additional space allowed between E and D = 3.

Then ED = 9, EC = 6, CD = 3.

Hence AE = 234, DB = 117, so that AE = 2DB

Fig. 10.09

in I.8, the bridges at E and D must be slightly higher than the others, to ensure firm contact between them and the string. Thus the ends of the real sounding lengths will be slightly to the left of A and E and slightly to the right of D and B. But as he shows in a part of III.2 which we considered earlier, this will make no difference so long as the surfaces of the bridges are segments of the surfaces of equal spheres; and we can still take our measurements from the centres of the four bridges.

The sounding lengths, represented by AE and DB, are in the ratio 2:1. Hence DB sounds an octave higher than AE. Suppose then that the bridge at E is moved to the left, to a point X, such that AE:AX is the ratio assigned to the first melodic step at the bottom of the attunement; and suppose that the bridge at D is moved to the right to Y, so that DB:YB = AE:AX. In that case the relation between the new notes will be the same as that between the original ones. YB will be an octave above AX, and we shall have constructed the first step in each of two *systemata*, one an octave higher than the other. When each of the eight strings has been divided like this at both ends, in the way appropriate to one of the notes in the octave, we shall have the complete series of fifteen notes represented on them. (In fact the eighth note will appear twice, once as the longer segment of the eighth string, once as the shorter segment of the first.)

These are the outlines of the procedure; they are sketched by Ptolemy at 86.1–15. But he is not satisfied. In this arrangement, the highest notes of the two octaves 'have poor resonance, particularly that next to B, because the segments that produce them are constrained by their shortness' (86.16–20; compare 83.9–11). He proposes for a second time,

therefore, that we should not begin with the strings all tuned to the same pitch, but

> again take precautions, making the four upper strings finer, equal in pitch to one another, but higher in pitch by a fifth than the four below them, these also being kept equal in pitch to one another. Thus a division up to only a fourth in both of the tetrachords, from the lowest notes upwards, will make the octave, this being put together from increase through a fourth on the basis of length, and increase through a fifth on the basis of tension. (86.20–27)

Since the divisions are represented in both segments of each of the strings, we shall again have constructed the double octave; and since the strings sounding the four upper notes of each octave are originally tuned a fifth higher than the others, their sounding lengths do not need to be shortened as much as they were in the unmodified procedure.

The gist of all this is clear enough. But the last sentence of the passage quoted above poses a tricky interpretative problem. It might be taken to imply that each of the strings in the higher group has a pitch exactly a fifth above its counterpart in the lower, and hence that the ratios within the two sets of strings are the same. But that would be so only if each octave were composed of two identical tetrachords separated by a tone; and even the lowest notes of the two groups will be exactly a fifth apart only if the tone disjoining tetrachords lies lower in the system than the fifth note (and in certain other rather special cases). Now the standard focus of attention in Book I was indeed an octave whose two tetrachords lie above and below a disjoining tone, and whose tetrachords are divided identically. Here, however, we are dealing with forms of the octave that lie at the top and bottom of the double octave, and in the fundamental, unmodulated *systēma*, they do not have precisely that structure (see pp. 165–6 above). More importantly, a concrete representation of the double octave can have a significant place in Ptolemy's procedure only in the context of the system of *tonoi*, in which the positions of tetrachords and tones within the two-octave span alter from case to case. Still more confusingly, a prime objective of his enterprise is to represent, on the strings of an 'experimental' instrument, the attunements used in practice by musicians. These not only differ from one another in *tonos*, but in several cases bring tetrachords with different divisions into the same system of attunement (see I.16, II.1, II.16). The important consequences are first, that in representations of the various *tonoi* the two groups of strings will not always correspond to two tetrachords between fixed notes, and in some cases neither of them will span exactly a fourth; that in some *tonoi* the interval between the note of any string and that of its counterpart in the other group will not be a fifth; and that in some pure *tonoi* and some practical tunings the ratios between the pitches of the strings in the lower

group will not be the same as those between corresponding strings in the higher. (It does at least remain true, however, that the upper and lower octaves in any *tonos* and in any generic mixture are always the same, so that the same ratio can properly be used to locate both of the bridges on any one string.)

The sentence at 86.23–7 then looks misleading. 'Thus a division up to only a fourth in both of the tetrachords, from the lowest notes upwards, will make up the octave, this being put together from increase in length over a fourth, and increase in tension over a fifth.' The immediate trouble is not with the word 'tetrachords'. It is used in this passage to mean nothing more specialised than either 'groups of four strings' or 'groups of four notes'; and though we shall have eventually to resolve the ambiguity, it need not concern us yet. But the sentence does seem to imply that the lowest string in the higher group will be pitched exactly a fifth above the first string; and it explicitly states that we need a division 'only up to a fourth' in both 'tetrachords'. (Here a 'fourth', *dia tessarōn*, is unambiguously a musical interval, not the ordinal number of some string.)

But we should not jump to the conclusion that Ptolemy is confused, forgivable though that might be in this tangled territory. The symptoms we have observed are better understood as indications that he is concentrating, once again, on representations of the attunements of practical music, not on the theoretical systems of the pure genera in all possible *tonoi*. For it turns out that in the 'practical' attunements attributed to musicians in II.16, though not in many others that could be constructed consistently with Ptolemy's 'pure theory', the interval between the lowest and the second-lowest notes in each octave of the complete *systēma* is always a 9:8 tone (though not in all cases a disjunction), and that between the second note and the fifth note, and between the fifth note and the eighth, the interval is invariably a fourth.[11] There are, I think, good musical reasons for this, but no mathematical ones. Hence it will indeed be true of all these systems that the notes contained in the upper group of four strings always span a fourth, and that the interval between the first note and the fifth note of the attunement is always a fifth.

We are not yet quite out of the wood. The interval between the first and fourth notes of these attunements is not invariably a fourth. Ptolemy speaks of 'a division up to only a fourth in both of the tetrachords'; and if 'both of the tetrachords' means 'both groups of four strings' (the sense of

[11] This can be seen from the table at *GMW2* p. 359. An octave of the relevant sort will run upwards from 'thetic' *mesē*, i.e. *mesē* 'by position', continuing upwards from thetic *nētē diezeugmenōn* with the three ratios allocated in that table to the three intervals at the bottom. This octave is identical with the one starting from the bottom of the double octave, thetic *proslambanomenos*.

'tetrachords' at e.g. 88.4), he is being inexact. But this need not be the sense. Ptolemy's focus in these paragraphs is exclusively on the higher notes of each octave. The problem was, specifically, that 'the highest notes of the two octaves – taken at the halves of AE and DB – have poor resonance, particularly that next to B' (86.17–19). We have been told to raise the pitch of the upper four strings through a fifth; and then comes the problematic sentence.

Now if 'tetrachords' here does not mean 'groups of four strings', but 'groups of four notes', the reference will be to the groups of notes Ptolemy has just been discussing, those sounded respectively by the longer and shorter segments of the upper strings, the highest four notes in each octave. (The statement that they 'make the octave' can quite naturally be taken to mean that they complete it or reach it, rather than that they constitute it.) In that case, what Ptolemy says will be correct. Apart from the general focus of the passage, there are two other pointers to this interpretation. First, what is 'put together from increase through a fourth on the basis of length, and increase through a fifth on the basis of tension' must be the set of pitches in the upper set of strings only. The description has no application to the lower. Secondly and straightforwardly, of the two other occurrences of the word 'tetrachords' in the part of II.2 that deals with the present construction, one is ambiguous (86.28), and the other quite certainly means 'groups of four notes', not 'groups of four strings'. It refers to 'the higher tetrachords, in the ratio 3:2 to those indicated by the table of numbers' (87.17–19). There is no doubt here that 'the higher tetrachords' are the groups of four notes at the top of the higher and the lower octaves, those represented in the two sets of segments of the strings in the higher set. It is a fair hypothesis, I think, that 'both of the tetrachords' at 86.24 is also a reference to both of two sets of four notes, not strings; and in the context these could only be the highest four notes in each octave. In that case, as I have said, Ptolemy is entirely correct.

We have had to struggle to reach this interpretation. If it is right, Ptolemy's remark at 86.23–7, which at first seemed careless or worse, would appear to reflect instead his meticulous attention to the details of the attunements he is principally concerned to capture, those of the musicians themselves. This comfortable conclusion may be shaken, however, by what he says a few lines later. 'When we construct the positions of the higher tetrachords, in the ratio 3:2 to those indicated by the table of numbers [those given in II.15], we must take care to ensure that we introduce them to the divisions taken at both ends of the measuring rod' (87.17–20). So far this is harmless. We are merely being reminded that we have to perform a mathematical operation on the numbers given in the tables that indicate the length of string proper to each note, in

order to get the right result when the strings are tuned a fifth above their counterparts. We are not to forget that these computations are needed to arrive at the right lengths for both the longer and the shorter segments of the strings. But the next sentence, in Düring's very probable reconstruction of the text from chaotic manuscript readings, is as follows. 'We shall extend these to $130\frac{11}{60}$ parts, so that we may be able to construct the number in the ratio 3:2 to that belonging to the lowest of the four notes that start from the highest, which comprises $86\frac{47}{60}$ parts' (87.20–88.1). The intention is clear when one looks at the tables in II.15. The number $86\frac{47}{60}$ represents the greatest length that is ever assigned to the fourth-highest string, in any combination of genus and *tonos*. Hence if we multiply it by 3/2, giving $130\frac{11}{60}$ to the nearest sixtieth, we find the greatest distance that has to be marked on the measuring rod used for constructing the divisions of the higher strings. This is all very well. The trouble is that a length of $86\frac{47}{60}$ units never in fact belongs to the fourth string from the top of the double octave. It is that of the fourth string from the top of the central octave in Mixolydian, in certain genera,[12] and one might have expected it to reappear in the upper and lower octaves of the fifth *tonos* in order from it, Hypolydian. In fact, however, the notes of Hypolydian are not at a fifth below their counterparts in Mixolydian, but at a distance of two tones and two *leimmata* (see II.10), and the corresponding lengths are quite different (see Table 12 in II.15).

This time Ptolemy really does seem to have been careless; and if his focus is on the attunements attributed to musicians, the mistake is compounded. Neither the Hypolydian nor the Mixolydian *tonos* is used in any of them (see II.16). Further, if Hypolydian were to be employed, then even ignoring the first mistake, there would be another problem, since the lowest string of the upper set could no longer be tuned at a fifth above the first string (II.15, Table 12). One way or another, Ptolemy's attention has wandered somewhere in this tricky passage. I suspect that the slip was only momentary, and that in identifying the greatest length to be assigned to the fourth string he merely glanced at his table of all the lengths that each string can have in any of the octaves (the lowest, central and highest octaves, Table 15 in II.15), and automatically picked the greatest number. If that mistake is eliminated, the rest is perfectly coherent, and is consistent with the hypothesis that his attention is still overwhelmingly focussed on the attunements of musical practice.

[12] See Table 1 in II.15, remembering that in Ptolemy's text and tables the numbers are rounded to the nearest sixtieth, but that this is not done in the versions given in Arabic numerals in Düring's edition and in *GMW2*.

Between the two sentences we have been studying, Ptolemy offers a formal demonstration that the ratios between any two pitches will be preserved when the overall pitches of the strings by which they are sounded are raised through a fifth, and more generally that the mode of approach he has recommended will preserve all the ratios proper to the division of the octave (86.28–87.16). Since the reasoning is straightforward and the conclusions are evidently sound, I shall not examine it further.

He now proceeds to another suggestion.

> The length belonging to the higher notes will be increased still further if we make the four notes in question a whole octave higher than the ones below them. In that case each of the two octaves will no longer be constituted, as before, by both tetrachords [i.e., both sets of four strings] together, but instead one will be constituted by one, the other by the other – that is, the whole higher octave by the higher tetrachord and the lower by the lower, the same division being aligned with each. (88.1–7)

Ptolemy pursues the idea in detail at 88.8–89.11. He suggests an arrangement which divides one of the strings in each set of four to give both the highest and the lowest note of an octave, the next to give the second-highest and the second-lowest, and so on. Hence we can run through the notes of the octave in order by plucking the segments at one end of strings 1, 2, 3, 4, and then those at the other end of strings 4, 3, 2, 1, 'so that the arrangement is contained in a circle' (88.15). Since the strings for the higher octave are initially tuned an octave above those of the lower, we need only one set of measurements on the rod in order to construct the notes in both of them – a minor practical convenience.

In the course of II.2, Ptolemy has presented three kinds of construction. In the first, eight strings are initially tuned to the same pitch. In the second and third they are split into two groups of four; in one case their initial pitches are a fifth apart, in the other an octave. He now sets out a couple of general reflections on their properties. First, he repeats his comment that in the last arrangement, the smallest lengths required will be greater than they are in the others (89.12–15). This is plainly true, and the last construction therefore has the advantage of giving the higher notes better resonance, the declared aim of the whole passage.

He next returns to the two principal forms of eight-stringed instrument, those derived respectively from the monochord and the *helikōn*, and their different kinds of bridge. 'It is also clear that with this method [where two sets of four strings are tuned an octave apart], only the first procedure can succeed, and that the one that works by means of common bridges is no longer possible' (89.15–17). The 'first procedure' is that which uses a separate moveable bridge for each string, and 'the one that works by means of common bridges' is the instrument

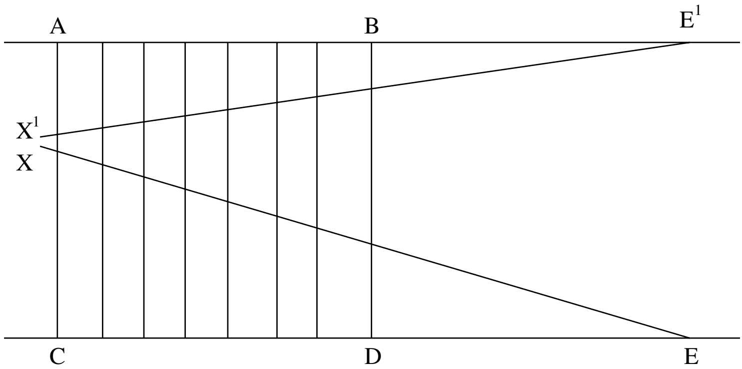


Fig. 10.10

related to the *helikōn*. As Ptolemy goes on to explain (89.17–25), the reason why the latter cannot be used with the last arrangement of strings is that the ratio between the sounding lengths at one end of two adjacent strings is not, in that arrangement, the same as the ratio between their sounding lengths at the other. The octave-series goes round 'in a circle' on each set of four strings, and the ratio between the relevant lengths of strings 1 and 2 in the first step of the scale, for instance, will not normally be the same as the ratio between their lengths at the opposite end when the seventh step is formed. In the instrument derived from the *helikōn*, the ratios between the strings' sounding lengths are determined by lateral movements of the strings themselves (p. 211 above); they are controlled by the distances between the strings. But the distance between a string and its neighbour must be the same along its whole length; and in the arrangement described above, some form of which is required when the two sets of strings are tuned an octave apart, this condition cannot be met.

That much is straightforward enough. It is less clear whether Ptolemy means to imply that the instrument with common bridges can be used when the sets of strings are tuned a fifth apart, or only when they are all tuned initially to the same pitch. In fact, both are possible, since in each case the two octaves are deployed in parallel at opposite ends of the strings, and the ratios involved at both ends will be the same. The only complication is that while only two continuous bridges are needed when the strings' pitches are initially the same, one for the lower octave and one for the higher (since the sounding lengths continuously diminish as we ascend each octave), in the other system we shall need four, two for each set of four strings (since the sounding lengths will increase again at the

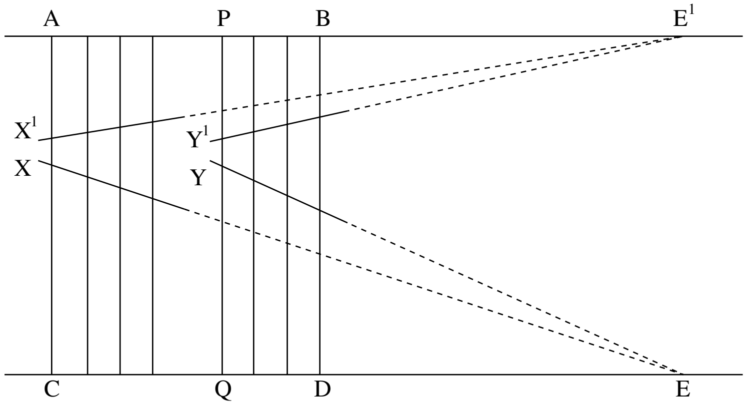


Fig. 10.11

beginning of the upper set). To clarify the point, and for the mere fun of the thing, I offer sketches of the two arrangements in Figures 10.10 and 10.11.

In Figure 10.10 (as in Figure 10.06) AC and BD, and the lines parallel to them, represent strings, initially tuned in unison. XE and X^1E^1 are bridges. $AB = BE^1 = CD = DE$. The measurements determining the lateral positions of the strings are taken from E and E^1, and are the same in both cases. So long as AX^1 is half XC, the positions of the bridges will make all the sections of string between AB and bridge X^1E^1 half the length of their counterparts between CD and bridge XE, so that the two octaves are identically tuned, no matter what the relative distances between the strings may be.

In the second arrangement (Figure 10.11) the basic principles are the same as in Figure 10.10. Strings PQ-BD are tuned initially a fifth above the other four. XE, YE, X^1E^1 and Y^1E^1 are bridges, where the dotted sections are only lines of construction, not physically present. As before, AX^1 is half XC, so that the notes sounded at the two ends of each of the first four strings are an octave apart. The same is true of the second four. Since string PQ is tuned a fifth above AC, and since, as we have seen (p. 220 above), the notes of PY^1 and YQ are always a fifth above those of AX^1 and XC respectively, PY^1 must be the same length as AX^1, and YQ as XC. The ratios of the octave contained in the segments of string between APB and the two upper bridges are the same as those in the segments between CQD and the lower bridges. But the ratios between the sounding lengths of the four upper strings in each octave, those from PQ to BD, need not be the same as those of the lower four. The pitches at both ends of the string

are determined, as in all uses of such instruments, by the lateral position of the string itself, their spacings being fixed by the ratios of the distances between their ends and E or E^1.

The only substantial issue dealt with by Ptolemy in the remainder of III.2 is one that we have already considered (pp. 200–1). It is time to review some of the points that have emerged in this rather complex chapter. One of Ptolemy's main concerns in all the passages we have looked at is to ensure that none of the distortions of pitch affecting other instruments is allowed to creep in at any stage. It is not, for the most part, the theoretical credentials of the geometrical plans of his devices that are at stake, but the practical reliability of the concrete pieces of apparatus themselves. He sets out by explaining how the monochord, and similar instruments, can be used to test the reliability of their own material components – an interesting early occurrence of the notion of a self-correcting apparatus. Later, as modifications and new instruments are introduced, Ptolemy invariably explains why it is that they bring no uncontrolled variables with them, or how such distortions can be eliminated in practice. His thorough examination of even very minor issues leaves few of these practical problems unresolved; we identified only one (pp. 198–9), and it is so marginal as to be of no practical consequence.

Several of the special features of the design of his instruments are dictated neither by mathematics nor by the need to eliminate distortions, but by their convenience when the instrument is put to use. This is true of tiny details, like the suggested addition of a second set of *kollaboi*. But Ptolemy's wish to make the instruments easier to manipulate is also what motivates, for instance, the elaborate accounts of different varieties of instrument in III.2; and the same principle underlies many of his comments on the relative virtues of the various devices and their bridging systems. All of them are mathematically respectable. What distinguishes them are their differing merits and degrees of awkwardness when used for different purposes; and Ptolemy has shown himself prepared to reflect on quite minute difficulties that might arise in practice.

It seems likely, furthermore, that he is not merely reflecting on the characteristics of existing instruments. At least in the case of the instrument derived from the *helikōn*, and in that of the constructions in III.2 (which may well have been prompted by the similar but much more rudimentary suggestions of Didymus), there is a strong possibility that we are looking at kinds of apparatus thought up by Ptolemy himself. In that case, since he concentrates as closely on their concrete features as on their geometrical design, we have some reason to believe that he actually had some such instruments built, to his own prescription. We have seen, very clearly in one example and to a lesser extent in others, that his descriptions read

more like excerpts from a construction manual than like passages from a text-book in geometry.

A rather different perspective on the matter is provided by a number of passages which, on the whole, I have alluded to without exploring in detail, since they involve individually no special problems or points of interest. They are of more importance when taken collectively. What they provide are arguments of a mathematical, usually a geometrical sort, designed to show that none of the requirements of theory have been overlooked when practical modifications are introduced. Often they are presented as supplements to less formal discussions of the same points. (Examples will be found, for instance, at 27.14–28.12, 83.22–84.10, 84.21–85.2.) At one level this procedure requires no justification. We obviously need to be sure, when altering our instruments or our ways of using them, for practical reasons, that we have done nothing to undermine the instruments' accuracy; and these formal demonstrations will give adequate reassurance. But they are also a reminder of the rather special characterisation which Ptolemy gave at the start for all these instruments and their role in the science. Perception is to be guided by reason 'towards distinctions that are accurate and accepted' (3.14). 'It needs, as it were as a crutch, the additional teaching of reason' (3.19–20). But since reason cannot inform our perceptual impressions directly, our hearing, like our eyesight, requires 'some rational criterion working through appropriate instruments, as the ruler is needed to deal with straightness, for instance, and the compasses for the circle and the measurement of its parts' (5.3–6). 'The instrument of this method is called the harmonic *kanōn*, a term adopted out of common usage, and from its straightening [*kanonizein*] those things in sense perception that are inadequate to reveal the truth' (5.11–13). In short, while the task of these devices is to display harmonic systems to perception, they can have this role only in so far as they are properly constructed 'instruments of reason'. It will be futile to bring mathematically derived harmonic divisions to an instrument unless we are certain not only that its abstract design is rationally grounded, but also that after its construction as a concrete piece of equipment, and after any modifications that we may have adopted, it still remains the faithful servant of reason. In cruder modern jargon, we must be sure that every detail of the structure of the input will be repeated in that of the output. But because it is these instruments' loyalty to mathematical reason that must be ensured, it is necessary to demonstrate it by formal, rational proofs, not just to be persuaded of it through intuition and unsystematic reflection. The rational credentials that the instruments claim are mathematically grounded, and only mathematical reasoning itself is competent to assess them.

Finally, we should notice the points at which Ptolemy's major discussions of instruments are located in his text, and the issues with which each is associated. The initial description of the monochord, and of the technique for testing its string (I.8), is a preliminary to the presentation to perception of the ratios of the concords. Ptolemy continues to maintain that the instrument is adequate for this purpose, but for little else. The simplest kind of eight-stringed instrument appears in I.11, along with the argument showing that its several strings, even if they have very different material constitutions, can properly be treated as having identical properties. It is to be used to demonstrate an elementary proposition about the octave and its constituent intervals. This kind of instrument remains unmodified through the rest of Book I, and is specifically referred to, with a brief explanation of its mode of use, when Ptolemy's generic divisions of the tetrachord are to be brought to the judgement of the ear (in I.15, at 37.5–12; see also 38.29–30).

Studies of more complex instruments begin in II.2, with the account of the *helikōn* and its derivative. As we have seen, the former, whose use extends only to demonstrating the ratios of the concords, is discussed mainly with a view to extending its principles into the construction of the latter. The immediate context is provided by the description, in II.1, of a way in which an eight-stringed instrument can be used to assess Ptolemy's analyses (I.16) of attunements used by musicians; and there are certain references in II.2 that point back directly to the arguments of II.1. Now the main topic of Book II is the *tonoi*, and as we approach the passages in which compendious tables of divisions by genus (II.14) and by genus and *tonos* (II.15) are presented, we might expect comments on the instruments to be geared to their role in offering to the ear the systems that these tables set out. But we have found that this is only part of the truth. In the discussion of the monochord's weaknesses (II.12) and its continuation into the critique of proposals made by Didymus (II.13), the main focus is consistently on the uses of experimental instruments to assess analyses of forms of tuning deployed in real musical practice. We may now recall that the analysis of practical attunements given in I.16 was incomplete; and the procedure for assessing it described in II.1 is incomprehensible without the later portions of Book II, where the *tonoi* are discussed and tabulated, and where particular *tonoi* are attributed (II.16) to each of the musicians' tuning systems. There are clear signs here – and we shall see more of them in Chapter 11 – that the principal purpose of the whole elaborate schematisation of *tonoi* is simply to make possible an adequate account of those practical attunements.

In this light it seems significant, also, that the ingenious and valuable instrument derived from the *helikōn*, whose description was prompted by

a passage about those attunements, then disappears from sight until the attunements of musical practice are again overtly considered, in II.16. It is here that we are told about the method of shifting the strings sideways on their sliding *pelekēseis*. The elaborate discussions of various ways of setting out a double octave on an instrument follow immediately (III.1–2); and there too we found clear indications that Ptolemy's attention is firmly set on ways of replicating the attunements used by musicians, on an instrument in which the ratios of their intervals can be accurately identified.

Nothing has been proved, but our investigations have suggested, broadly speaking, two provisional conclusions. One is that Ptolemy's accounts of the instruments are designed to make it possible not only to appreciate their rational credentials, but to build them and use them. The second is that while different instruments may appropriately be used, and different elements of them combined, for various different purposes, the whole thrust of the exercise is towards the development of instruments capable of presenting to the ear's judgement proposed quantifications of the attunements of musical practice. We have seen a few signs – quite apart from his explicit protestations – that Ptolemy himself had some experience in conducting such tests. If this is right, it is clear that they would have to be real tests. In the way he has approached musicians' practices, expecially in I.16, he has left himself no room to argue that if his attunements fail to match theirs, it is theirs that are at fault. If, as now seems highly probable, he did represent his analyses of their systems on the strings of an instrument, he could not have avoided putting his accounts of them at risk, and with them the 'theoretical' systems on which they are based. It begins to look as if his programme of 'empirical testing' should indeed be taken seriously.

11 The tests

The questions to be considered in this chapter overlap with those of Chapter 10, but we shall take a slightly different angle of approach. There we based our discussion on what Ptolemy says about his instruments; here we shall concentrate on his account of what we are to do with them when we have got them. Broadly speaking, his comments fall under three headings. Some are concerned with the preparation of the data, that is, with the way in which propositions to be assessed must be expressed, if their content is to be 'displayed to perception' on an instrument's strings. Others relate to the procedures by which attunements are to be set up in practice on the instruments themselves, and by which the constructions of reason are actually to be made accessible to perceptual judgement. Finally there are passages that make statements or carry implications about the criteria according to which such judgements can be made. The thesis that they are made 'by perception' or 'by the ear' is altogether too vague, and we must see what efforts Ptolemy makes to sharpen it up.

The relevant passages are of course not separated out under these headings in the text. In reviewing them there are several important questions that we shall be seeking to answer. More or less tentative answers to some of them have been proposed in Chapter 10, and in these cases we shall be looking for further evidence that might bear on these provisional conclusions. The question whether Ptolemy really used any instruments at all, or intended his readers to do so, has already, I think, been settled with some certainty; but various points providing additional confirmation will be mentioned as we go along. It still remains possible, however, despite his explicit pronouncements, that like most of his predecessors, Ptolemy conceived the presentation of propositions in perceptible form more as a strategy for displaying the truth of his conclusions than as a way of submitting them to experimental tests. This issue, it seems to me, is much the most important we shall be facing here. Some reasons have already been given for taking the suggestion of testing seriously, but we must see how it fares when confronted with evidence of a different sort.

We must also ask whether the way in which he prepares his data

encourages or discourages the belief that the resulting formulations were actually used for the purposes described, and if they were, whether they would have represented the data adequately. Again, we need to reconsider the relations between attunements derived from *hupotheseis* and those attributed to musicians. We should try to decide whether both groups are supposed to be perceptually assessed in the same way, and whether the reliability of one of these sets of tests is in any way dependent on that of the other. We must also revisit the question whether Ptolemy's main purpose is to offer to perceptual judgement his analyses of theoretically correct systems, or those of the systems used in practice, or whether equal emphasis is placed on both. Finally and most generally, we must ask how well his strategies are adapted to his declared aim of 'saving the rational *hupotheseis*'. I do not propose, however, to go through all these questions and the evidence bearing on each of them one by one. That would involve too much tedious recycling of the same passages. Instead I shall again follow roughly, but not exactly, the order in which the passages I think relevant appear in Ptolemy's text, and see what enlightenment each has to offer on any or all of the issues I have mentioned. The results of that procedure will, I think, be rather less messy and disorganised than one might reasonably fear at the outset.

By way of a thread on which to hang the excerpts we shall take from Book 1, I want to suggest in advance that the evidence they provide to support the view that the empirical tests are genuine becomes progressively more compelling as the book proceeds. That is itself, of course, a hypothesis to be tested. We begin with a sentence at the end of 1.7, when Ptolemy has completed his theoretical derivation of the ratios of the concords. 'But now it would be a good thing to demonstrate [*apodeixai*] the clear truth of the ratios that have already been set out, so that we may have their agreement with perception established beyond dispute as a basis for discussion' (16.29–31).

The 'demonstration' in question is described in 1.8, after Ptolemy's preliminary reflections on the inaccuracy of various instruments, and his account of the structure and credentials of the monochord (see pp. 196–202 above). The core sense of the verb *apodeixai* is 'to display', 'to exhibit', and especially in philosophical or scientific contexts it is regularly used to mean 'to prove', 'to show by argument'. An *apodeixis* can be the 'exhibition' or 'exposition' of something, but in technical writings it is the commonest word for 'proof', especially one set out in strict logical form. It carries not the least suggestion of testing a proposition or trying out a hypothesis. In the present context the senses of 'display' and 'proof' are combined; it is by having what they claim to be facts displayed to perception that the propositions Ptolemy has enunciated will be 'established

beyond dispute'. The same view of the matter is plainly reflected in his account of the demonstration itself. After we have set up the apparatus and assured ourselves that the string we are using is a true one, 'when the measuring rod has been divided in the ratios of the concords that have been set out, by shifting the bridge to each point of division we shall find that the differences between the appropriate notes agree most accurately with the hearing. For if distance EK [from a fixed bridge to a moveable bridge] is constructed of four such parts as those of which KH [from the moveable bridge to the other fixed bridge] is three, the notes corresponding to each of them will make the concord of a fourth through the ratio 4:3'; and so on for the other ratios (18.23–19.15).

We should not be surprised by these indications that the procedure constitutes a 'display' or 'proof', and not a test. The ratios of the concords had been known for centuries. Even in modern scientific works it is common to meet descriptions of operations couched in similar terms – 'if we do such and such, we shall observe that . . .' – especially when the proposition whose truth is alleged to be exemplified in the results is one regarded as uncontroversial. It would be strange, in fact, if Ptolemy showed signs of construing his demonstration as some sort of test, such that if the result came out wrong on some occasion, that would cast real doubt on the correctness of the ratios. Their values were by now so well established, in the tradition of mathematical harmonics, that the only proper response to an inappropriate result would be to assume that the apparatus had been wrongly set up, and to look for the fault. The case will of course be different when Ptolemy comes to his own special evaluations of the melodic ratios, since these are genuinely controversial, and unsatisfactory responses by the ear, when the postulated divisions are presented to it, could not automatically be dismissed as due to inaccuracies in the instrument, or in the ear – not, that is, if Ptolemy's statements about submitting *hupotheseis* to perceptual judgement are made in good faith.

Before leaving I.8, we should notice one minor point about the way in which the ratios are to be displayed on lengths of string and in the resulting pairs of sounds. Given one string and one moveable bridge, there are two ways in which it can be done. In one of them, the length of the whole string, without the moveable bridge, is used to give the lower note in any pair. The note a fourth above it, for instance, will be found by placing the moveable bridge three quarters of the way along the string, and plucking the longer section. In the other, the two notes in each relation are produced by the lengths of string on either side of the moveable bridge, so that the interval of a fourth is formed when the bridge is four sevenths of the way along the string from one end, and three sevenths from the other.

Mathematically, the first method is simpler and neater, since the positions of the moveable bridge corresponding to the ratios of all concords within the double octave can be found on the basis of divisions of the string into halves, thirds, quarters and in one case eighths, whereas the second method requires the construction of thirds, quarters, fifths, sevenths and in one case elevenths of the whole lengths. For empirical purposes, however, the second is greatly to be preferred, since the two notes of each concord can be sounded together, or in quick succession, without moving the bridge, so that their relationship can be more readily grasped and appreciated by the ear.

Unlike some other writers,[1] Ptolemy chooses the second approach. This fact gives some support to the view that he intended serious students of the subject to 'display' the ratios of the concords to their ears in practice, even if the operation is not to be understood strictly as a test. Writers who take the other route are typically more concerned to represent to the mind the mathematical simplicity and orderliness of the ratios.[2] The form of Ptolemy's procedures fits well, then, with the concerns that seem to motivate the careful attention he has given, earlier in I.8, to the physical details of his instrument.

The pattern of discussion in I.11 points even more directly to conclusions of this sort. Here Ptolemy is seeking to show that the octave is not equal to six tones, but is slightly less. It is part of his polemic against Aristoxenian conceptions, according to which the fourth is exactly two and a half tones, the fifth three and a half, and the octave six. We should notice first that though the Aristoxenians reject the representation of intervals as ratios of numbers, their musical definition of the interval of a tone coincides with Ptolemy's (and is in fact accepted by all theorists). The tone is the difference between a fourth and a fifth.[3] From Ptolemy's point of view, the ratios of the concords have been firmly established, and it follows from them, together with the accepted definition of the tone, that its ratio is 9:8. Hence he can insist that even if the Aristoxenians refuse to treat this way of representing intervals as musically significant,[4]

1 For discussion, see Barker (1991), especially pp. 56–67.

2 In addition to the passages discussed in Barker (1991), see particularly the report on Adrastus at Theon Smyrn. 57–9. Here the author constructs all the concords from the whole string, its halves, thirds and quarters, since he ignores the octave plus fourth (8:3) which would involve eighths; and he continues by commenting that 'all the concords are contained in the *tetraktus*, since this is composed of 1, 2, 3 and 4'.

3 For an Aristoxenian statement of the definition, see e.g. Aristox. *El. harm.* 21.20–23, and for a 'Pythagorean' statement, [Eucl.] *Sect. can.* prop. 13.

4 Aristoxenus' own position does not commit him to treating the propositions of mathematical theorists as false. But on his view they belong to a science (that of physical acoustics) which is distinct from and irrelevant to the business of musical harmonics. See especially *El. harm.* 12.4–19, 32.18–28.

they must nevertheless agree that what they call a tone is identical with what he calls an interval in the ratio 9:8; and in that case, if he can show that this interval repeated six times exceeds the octave, he will have made his point.

His first formal argument (25.16–26.2) is purely mathematical, though its details are not presented in full.[5] This kind of 'demonstration' has nothing to do with the representation of ratios on a string or strings. We are not required to use our ears, or to make observations of any sort, only to do sums; and Ptolemy is perfectly clear about the distinction. The opening of his next paragraph indicates a transition to a second type of demonstration. 'This sort of result will easily be grasped if we fasten seven more strings on the *kanōn*, in association with the one string, on the basis of the same kind of selection and placing' (26.3–4).

Procedures with the instrument, then, are to be undertaken in order to supplement the mathematical argument, and are quite sharply distinguished from it. This clear division between purely 'rational' arguments, on the one hand, and demonstrations that are based on rational principles but which convince us through impressions made on the senses, is further evidence of Ptolemy's intention that demonstrations of the latter kind should actually be performed. They are not just more mathematics, tricked out with colourful references to perceptible instantiations of the relations that are mathematically manipulated. As it turns out, the present demonstration is not an arithmetical one at all, but depends crucially on the way in which a relation formed on the strings is found to strike the hearing.

The procedure is straightforward. Once we have set up eight strings, equal in length and pitch, we are simply to construct on them, by use of the measuring rod, six successive intervals upwards in the ratio 9:8, and then, from the same starting point, to construct one interval upwards in the ratio 2:1. We then compare, by ear, the highest note reached after six steps of 9:8 with the higher of the notes in the ratio 2:1. The former, we are told, will sound slightly higher than the latter (26.5–14).

Ptolemy's presentation shows signs similar to those in the earlier passage of his treating the procedure as a demonstration, rather than a test. He does not say that if the one note sounds higher than the other it will confirm his proposition, but asserts, unhypothetically, that this is the result we shall find. We might wonder, perhaps, what exactly he intends by the expression 'easily grasped' (*eukatanoēton*) in the sentence I quoted above (26.3–4). If it meant something like 'easily understood',[6] it might be worth asking whether Ptolemy's procedure could be interpreted as

[5] For a fully worked example of the argument see [Eucl.] *Sect. can.* propositions 9 and 14.
[6] This was my rendering in *GMW2* p. 300.

something more interesting than a mere demonstration. There would still be no suggestion of a deliberately constructed test; but there might be a hint that through the empirical method we can come to understand why it is that the arithmetical calculations produce the results they do. But this interpretation seems unlikely. Though the verb *katanoein*, from which Ptolemy's adjective is derived, can indeed mean 'to understand', it is regularly used by authors to whom such distinctions matter in the sense 'to grasp through perception', and this is probably its meaning here. (Ptolemy himself is clearly using it in this way, for instance, at 37.6–7.) The procedure is then conceived in much the same light as that of 1.8. Just as that one was preceded by a description of the monochord and the method for testing its string, so this one is introduced by a (rather sketchy) description of the new instrument, and immediately followed by the argument showing that its eight strings, when properly set up, have equivalent properties (26.15–28.12). As before, Ptolemy plainly intends that the apparatus should really be constructed and the empirical judgement made. But he thinks of the procedure as 'displaying' the truth of the proposition in question, not as testing a hypothesis that might be false.

The first suggestion of a step away from mere demonstration in the direction of experimental tests appears in 1.14. Ptolemy is commenting on the generic divisions proposed by Archytas, whom he has represented as devotedly trying 'to preserve what follows the principles of reason', but as having gone badly astray, since at several points his divisions come into conflict with 'the plain evidence of the senses'. In the context of Archytas' declared adherence to rational principles, these conflicts

> seem to set up a slanderous accusation against the rational criterion, since when the division of the *kanōn* is made according to the ratios set out by his proposals, that which is melodic is not preserved. For the majority of those [divisions] set out above, and of those that have been worked out by virtually everyone else, are not attuned to the characters generally agreed on. (32.10–15)

Now the notion of a division's 'character' (*ēthos*) is admittedly vague, and though Ptolemy uses the word in similar contexts on several other occasions, he never attempts a close analysis of the ways in which *ēthos* and quantitative form are related. (For relevant passages see 29.1–2, 38.4–5, 30–31, 44.6–7, 55.7–9, 58.13–20.) But two things are clear. One is that the *ēthos* of an interval or a system consists in the kind of aesthetic impression it makes on our senses, including not merely its perceptible sound-patterns but also the affective characteristics they bring with them (29.1–2). The second, reflected in all the passages cited above, is that in one way or another *ēthos* is indeed dependent on mathematically expressible form. What Ptolemy is saying here, then, is that when pitches are

placed in sequence according to the ratios proposed by Archytas or by the other theorists mentioned, they neither sound the way we expect sequences in these genera to sound, nor convey the kind of feeling that is generally agreed to be associated with them.

These criticisms are wholly distinct from the others that Ptolemy has made, and will make, in the remainder of 1.14. Apart from one minor and very specialised comment directed exclusively at Aristoxenus (32.25–7), all of the latter concern alleged breaches in the rules that govern all divisions alike. The suggestion that a division is wrongly formed because it fails to convey the impression proper to the genus to which it is assigned is of a different sort altogether, and except in grossly obtrusive cases it is not one that could be substantiated just by allusion to rules. Ptolemy makes no attempt to insinuate that it can. It is 'when the division of the *kanōn* is made according to the ratios set out by his proposals' that it becomes evident that these ratios deliver the wrong characters.

At a general level, we are left with only two ways of interpreting the passage. One is to accept that Ptolemy actually did what his words suggest – that he set out Archytas' ratios and those of other theorists on the strings of an instrument, listened to the results and found them aesthetically inappropriate. The other is to dismiss his remarks as empty rhetoric, based on nothing but the prior conviction that these ratios cannot be the correct ones. In the former case, no matter how prejudiced he may in fact have been before making his tests, and no matter how bad a judge he may have been (for that reason or any other), he was conducting an experiment, and one that could have been repeated by more impartial investigators. This remains true even if Ptolemy, when writing his text, is presenting the canonic procedure as a demonstration or 'display' of the falsity of Archytas' claims. That is precisely the light in which we would expect a scientist to report the results of experiments, where their results had been inconsistent with the hypotheses they were designed to test.

If we adopt the second view, we are committed to accusing him not only of making unsubstantiated assertions but of dishonesty, since he is patently claiming that what justifies his comments is the ear's response to the sounds of an instrument. I see no good reason for adopting this interpretation, though I concede that it is still open to sufficiently sceptical minds. There is a strong case, to put it no higher than that, for the contention that the passage gives evidence of something genuinely conceived as an empirical experiment. Similar considerations will apply to some of the comments Ptolemy makes much later, when criticising the divisions of Didymus in a passage we have already discussed (68.15–69.8, especially 68.32–69.8; see pp. 129–31 above). But these two passages are concerned, of course, only with propositions put forward by Ptolemy's rivals

and predecessors, and in that connection he would presumably have been happy to admit whatever contrary evidence came to hand. This is not enough to show that he was equally prepared to allow the same kinds of procedure to sit in judgement on constructions of his own.

We must now revisit some passages late in Book I, where Ptolemy discusses the relations between the divisions derived from his rational *hupotheseis*, and the impressions received by the ear. As soon as the divisions are complete, towards the end of I.15, he offers a confident assertion.

The fact that the divisions of the genera set out above do not contain only what is rational but also what is concordant with the senses can be grasped, once again, from the eight-stringed *kanōn* that spans an octave, once the notes are made accurate, as we have said, in respect of the evenness of the strings and their equality of pitch. For when the bridges set underneath are aligned with the divisions marked on the measuring rods placed beside them – the divisions that correspond to the ratios in each genus – the octave will be so tuned that the most musical of men would not alter it any more, not even a little. (37.5–12)

The appeal is unambiguously to perception. No study of arguments in a written text can give us the necessary kind of assurance that Ptolemy's divisions are correct. We can get it only if we ourselves go through the practical process of setting up an instrument in the proper way, dividing the strings in the ratios under consideration, and listening to the results. Plainly, Ptolemy is expressing the conviction that we shall be satisfied with them. Hence he seems once again to be thinking in terms of demonstrations rather than tests.

But the rhetoric of the second sentence suggests that he is, as it were, daring us to disagree, on pain of showing ourselves to be less than expert musicians. The challenge is amplified in the sequel.

We would be astonished at the nature of the ordering of attunement, if on the one hand the reasoning that deals with it moulded, as it were, and shaped the differences that preserve melody, and if hearing followed the lead of reason to the greatest degree possible (lying in this way alongside the ordering arising from reason, and recognising the appropriateness of each of its propositions), while on the other hand the outstanding experts in the subject condemned it, though they are unable, by themselves, to initiate an investigation of the rational divisions, and neither do they think fit to try to discover those that are displayed by perception. (37.12–20)

In the context this seems an odd piece of reasoning. Ptolemy does not say that it would be strange if reason dictated certain harmonic divisions and hearing nevertheless rejected them. He adopts as premisses both the supposition that 'reasoning shapes the differences that preserve melody', and the supposition that hearing 'follows the lead of reason' and

'recognises the appropriateness of each of its propositions'. We can agree that if both these suppositions are true, it would be not only astonishing but absurd for 'outstanding experts' in music to reject the joint testimony of these two criteria. If reason dictates them and hearing accepts them, there can be no relevant grounds at all for disputing their claims. From one point of view, then, Ptolemy's remarks do not sound promising. He appears to be presupposing the correctness not only of his divisions, but also of his own general view about the relations between reason and perception.

But from another angle, what he says gives grounds for the belief that he seriously expected his readers to put his propositions to the test. Its role is to remind them of what is at stake, of what they will be committing themselves to rejecting if they claim, when they listen to his divisions, that they are unacceptable to their ears. They will have either to reject the thesis that reason is competent to determine correct divisions, or to allow this but postulate that hearing does not follow reason's lead (in which case what is 'musically rational' will have nothing to do with real music); or if they accept Ptolemy's view on both these issues they will necessarily be implying that his reasoning itself is faulty, in which case it will be incumbent on them to show where the mistakes occurred, and to 'initiate an investigation of the rational divisions' for themselves. The only remaining possibility is that their pose as 'experts' is a sham.

But they can be forced into this corner only if they have accepted Ptolemy's invitation to try out his divisions on an instrument, and if they then claim to find them perceptually inadequate. The rhetoric of this closure to I.15 is that of an advocate for the defence, and is patently designed to dissuade the jury from bringing in an unfavourable verdict. But it only makes sense in that light if the jury has some evidence to consider, and can in principle return whatever verdict it chooses; and the only evidence that is being admitted at this stage of the enquiry is that spoken by an instrument into the jury's ears. The implication, I suggest, is that we are indeed being encouraged to conduct a genuine experiment, analogous to those by which Ptolemy had assessed the divisions of Archytas. There is no reason to be disturbed by the fact that we are also being exhorted to take a particular view of the experiment's results. Ptolemy has already satisfied himself that his analyses are both rationally impeccable and aesthetically appropriate, and having done so he sees no reason to pretend to think that they might turn out otherwise.

We saw earlier, however (p. 145 above), that these conclusions are called in question by the opening of I.16. The experiments we have been asked to conduct, if such they are, have required us to pass aesthetic judgement on divisions which, even if they are correct, are neither

'familiar to our ears' nor ones whose characters (*ēthē*) we shall 'altogether enjoy' (38.1–5). We might fairly be puzzled as to how the ear can mark a distinction between musically correct ways of dividing the tetrachord which nevertheless create structures whose sound and feeling we neither recognise nor enjoy, and on the other hand divisions that are simply unmusical. Yet if it cannot, the suggestion that we should test these unfamiliar divisions through experiments with an instrument seems empty. The difficulty is quite serious, and a related problem will arise also in Book II, in connection with the *tonoi* (see pp. 255–6 below).

In the present case a solution of sorts is quite readily found, though it is one of which Ptolemy himself gives no hint. It is that even if the ear is incompetent to judge these arcane divisions, nevertheless its recognition of the perfect adequacy of others, derived by Ptolemy from the same *hupotheseis* and by the same method, could give grounds for faith in the credentials of his procedure as a whole. The 'familiar' structures would then fall into place as elements in a single, intelligibly coherent system to which others also belong, though they are used less or not at all in musical practice. Thus the proposition that what perception accepts as musical is so because it is rationally well formed receives its confirmation not from an exact, one to one match between what is aesthetically agreeable and what is rational, but from a demonstration that the former corresponds to one segment of the latter's more extensive domain. In that case the credentials of the unfamiliar divisions will not be assessable directly by listening to them, but will be confirmed indirectly by our acceptance of others that belong to the same rationally constructed series.

A second and more straightforward comment is also in order here. It must be reckoned as persuasive, though not conclusive evidence of the sincerity of Ptolemy's appeals to perception, that he does not offer the glib assurance that all his divisions will sound equally pleasant, or that all will be found to fit smoothly with the norms of familiar musical practice. Some of them will in fact strike the listener as audibly offensive. Even if he is merely trying to disarm the criticisms of those who might discover that fact, that is enough to show that he genuinely anticipated that the divisions would be offered to the judgement of the ear, and that those who undertook this task could be expected to do so in a critical spirit.

The next part of I.16 concerns what Ptolemy will call the 'even diatonic'. The ratios assigned to it are 10:9, 11:10, 12:11; and it will be recalled that it is not one of those derived in the regular way from Ptolemy's *hupotheseis*. His account of the reasons for accepting it has several interesting features. It begins, as we saw at an earlier stage (pp. 148–9 above), from reflections on certain formal characteristics of the tense chromatic division, those that are held to be responsible for the

'agreeable' impression it makes on the hearing. The most important of these characteristics is the 'evenness' of its initial division of the fourth into 7:6 and 8:7. This 'suggests' to Ptolemy the possibility of a different division, in which this evenness is extended to all three ratios of the tetrachord; and given the resulting triad of ratios, it is extended still further when the ratio of the disjunctive tone, 9:8, is placed above such a tetrachord (38.12–27).

Now Ptolemy raises the question of how this division strikes the ear.

> When a division is taken in [strings] of equal pitch on the basis of these numbers, the character [*ēthos*] that becomes apparent is rather foreign and rustic, but exceptionally gentle, and the more so as our hearing becomes trained to it, so that it would not be proper to overlook it, both because of the special quality of the melody, and because of the orderliness of the division. Another reason is that when a melody is played in this genus by itself, it gives no offensive shock to the hearing, which is true, pretty well, only of the intermediate one of the diatonics, among the others . . .' (38.29–39.1)

We have looked at some aspects of this passage already (pp. 148–9). The principal point I want to make here is that it contains no suggestion that this division is one in familiar use by musicians, nor is that implied elsewhere in the text. In fact the division is not mentioned again, except in so far as its ratios are listed, along with the others, in the compendious tables of II.14. If Ptolemy's form of exposition is to be relied on, it was 'suggested' to him initially by purely theoretical considerations. When presented to the ear it is found to have a certain charm, and pleasing melodies can be played in it, even if its character is not strictly Greek. At the end of the paragraph he does not say: 'and this is what is called the "even diatonic"', as though he had identified the form of another, generally recognised attunement. He says: 'so let us call this genus the "even diatonic", from the characteristics it has' (39.5–6). The implication seems to be that his reflections have led him to a new variety of division, one that the ear enjoys, but not one already found in practical music-making, or represented, accurately or otherwise, in the theoretical text-books.

Ptolemy claims, then, to have devised this division on the basis of 'rational' considerations suggested to him by another case. He tried it out on his strings, and was apparently so intrigued by the results that he persisted until his hearing had 'become trained to it'. But on this occasion he had no special axe to grind. If the division had proved audibly unacceptable the fact would in no way have undermined his *hupotheseis*, since it is not derived from them in the regular way, nor would it have conflicted with anything he goes on to say about the music of practice. The passage has all the appearance of being a report of an unbiassed piece of experi-

mentation, designed straightforwardly to test a theoretical possibility. To Ptolemy's ears at least, the experiment showed that it was equally an aesthetic possibility, something that was perceptibly agreeable and capable of being used as the basis of pleasing melodies. But its acceptability was not entailed by the theory, and if the results of the experiment had been different, no harm would have been done. The only consequence, I suspect, would have been a tactful silence; the division would not have been mentioned at all. I can see no reason to suppose that the experiment was not conducted, and conducted in good faith.

The situation is exactly reversed in the case of the 'ditonic diatonic' discussed at 39.14–40.20. We looked earlier at the way in which Ptolemy reached his conviction that this division was in practical use (pp. 153–5), and we need not consider the details again. But it is clear that on this occasion he began from observation, and found that what it showed did not square directly with the requirements of his *hupotheseis*. By his own rules of procedure he was committed to accepting the division's legitimacy, and was therefore compelled to find some plausibly 'rational' credentials for it. While they are apt enough, there is a detectable smell of the *ad hoc* about them, as we saw. Since Ptolemy treats this ditonic diatonic as in effect a deviant form of the rationally derived tense diatonic, it seems likely that he came upon the former kind of attunement while trying to discover how well his quantification of the latter fitted the perceptible melodic systems that it was designed to represent. He found that it came close, but that there were good, empirically grounded reasons for admitting that the fit was not exact. In that case his acceptance of the ditonic diatonic, and his provision of an account of its special kind of 'rationality', constitute a manoeuvre aimed at mitigating the difficulties arising from a 'test' whose results fail to chime perfectly with the proposition that is being assessed. We may applaud his resourcefulness or criticise the slipperiness of his vaunted allegiance to principle, according to taste. But on either view we must apparently recognise that he was prepared to accept, at least in this special case, that the results of his empirical investigations were inconsistent with the predictions of his *hupotheseis*, in their original, unmodified form. The *hupotheseis* are not therefore abandoned, but they are undeniably bent; perceptual tests have been permitted to exercise the right of adjudication which Ptolemy's declared methodology assigned them.

Ptolemy's reflections on the ditonic diatonic arose from his attempts to analyse the systems of attunement used by players of the *lyra* and the *kithara*. The reason for his focus on stringed instruments, to the exclusion of wind, is made clear in a later passage, where he is talking about the role of the monochord.

In *lyrai* and *kitharai* the melodic intervals were constituted according to the proper ratio, but this is not demonstrated on them, given that such a thing is not even achieved with accuracy on *auloi* [reed-blown pipes] or *syringes* [Pan-pipes], instruments that would have a completer competence at both sorts of presentation, since they construct the differences between notes in conformity with lengths. (66.19–24)

Though in these stringed instruments the notes' relative pitches are not correlated with lengths, but with degrees of thickness and tension, the pitches they sound are precise and can be attuned as accurately as the performer's ear permits. In wind instruments, by contrast, pitch is mainly dependent on length, but is affected by other factors too, so that a performer's pitching of notes in a melody is unlikely to be consistent and clear-cut.[7] Hence it will not be possible to give precise quantifications for the systems of attunement used by wind players. They cannot be read off from the structure of the instruments, any more than can those of the *lyra* and *kithara*, but neither can they be matched by ear with a scientifically constructed counterpart, since they are too vague and variable for us to decide exactly which pitch-relations are conceived by the player as correct. With plucked instruments, the precision of the pitches makes this latter kind of comparison relatively easy. We can determine with some confidence whether or not they correspond to those reproduced on a scientific instrument, tuned to a specific, mathematically defined pattern of ratios. But no accurate assessment of their attunements would seem to be possible in the absence of such comparisons, since no amount of listening to *lyrai* and *kitharai*, and no direct measurements of their parts (or not ones that Ptolemy could have made) will yield quantitative representations of their form. Instruments such as the monochord and its more complex derivatives have an indispensable empirical role.

Ptolemy represents the forms of attunement used by string players as involving the generic divisions that he has already described as 'familiar to perception', but in most cases as combining divisions of different sorts within the same attunement (initially at 39.6–14). He nowhere attempts to find a theoretical justification for their use of just these combinations, or indeed for the practice of combining different divisions in general. He remains content with the aesthetic observation that (apart from his own novelty, the 'even' diatonic) only the tonic diatonic, when used unmixed, 'gives no offensive shock to the hearing, . . . the others being attuned by forcible constraint when taken by themselves' (38.33–39.2). These others, in fact, can only be used acceptably when their tetrachords are combined in a special way with tetrachords in the tonic diatonic

[7] Compare 16.32–17.7, [Ar.] *Problems* XIX.43.

(39.2–14). The point of Ptolemy's analysis, then, is not to show that every aspect of musicians' practice can be accounted for by derivation from rational *hupotheseis*, but rather more modestly, to show that every structural element in the attunements they use corresponds precisely to one of those which have been rationally derived. The music of performance involves the selection and recombination of rationally accredited tetrachordal patterns; and even if the criteria by which it chooses its selections and combinations appear scientifically arbitrary, at least it employs no ingredients that reason would reject. This, of course, is why he spends so long on discussing the credentials of the ditonic diatonic, since the tetrachord characteristic of it is undeniably used, and it is the one such ingredient that is not, on the face of it, consistent with the principles guiding the 'rational' phase of Ptolemy's procedure.

When the musicians' attunements are first introduced, in I.16, one might easily get the impression that the business of providing quantitative analyses for them is merely incidental to Ptolemy's main concerns; but a suspicion that this impression is mistaken arises as soon as we turn to the opening of Book II. Ptolemy at once makes clear – clearer, it must be said, than he did in I.16 – how his analyses are supposed to have been reached and how their accuracy has been confirmed. The mathematical form of the divisions of their component tetrachords was derived from his rational *hupotheseis*, in the way we examined earlier. Then, after these generic divisions have been assessed empirically, one by one, the patterns of their combinations which Ptolemy has proposed have in turn been checked against the evidence of the ear. By the standards of Ptolemy's declared methodology, that should be enough. But at the beginning of II.1 he proposes a wholly different method of reaching the same results, and it is pursued in close detail through the remainder of this long chapter.

> There is also another way in which we can find the same sets of proportions, those of the genera that are familiar and readily accepted by the ear, not generating their differences from what is rational alone, as we did just now, and then submitting them by means of the *kanōn* to evidence drawn from what is perceived, but reversing the procedure, first setting out the attunements put together through perception alone, and then showing from them the ratios that go with the equalities and differences between notes that are adopted for each genus. (42.1–7)

This proposal is both novel and interesting. It suggests that instead of proceeding from theoretical derivation to empirical test, we can instead set off by an empirical route, setting up the attunements of musical practice by ear alone, and then proceed by some form of demonstration or argument to an accurate account of their mathematical form. The suggestion is the more intriguing for the extreme parsimony of the theoretical commitments which Ptolemy says we shall have to make in advance. 'We shall

assume here too only those things that are straightforwardly agreed by everyone, that the concord of a fourth bounds an epitritic ratio [4:3], and that the tone bounds one that is epogdoic [9:8]' (42.8–10).

Despite superficial similarities of presentation, it would be a mistake to suppose that this 'reversed method' is the same as the one sketched in a part of III.1 that I referred to earlier (pp. 213–14 above).

In this way an attunement can be made even by someone who is capable of assessing only strings of equal pitch; and on the other hand, it will be possible for a person who can accurately discriminate the correct differences between the notes in respect of each species to perform the reverse process – that is, no matter what pitch the notes may have, to set up the division belonging to any one genus and *tonos*, and then to attune the strings to the hearing in a way that conforms to what is laid down. (84.11–16)

In the procedure introduced by this quotation, as Ptolemy goes on to explain, we can check the accuracy of the analysis which determined the positions of the moveable bridges, once the strings' tensions have been adjusted so that the attunement satisfies the ear, by removing the bridges, or placing them in identical positions under each string, and discovering whether the strings are now tuned to equal pitches. If they are, the analysis was correct (84.16–85.8). This procedure is perfectly sound. But it is quite different from the method pursued in II.1.

We do not need to work our way in detail through the whole of this chapter. The approach that Ptolemy adopts is essentially the same at each step, and one example will be enough. Since some of the later operations depend on the results of earlier ones, it will be simplest to consider the one with which Ptolemy begins.

Of the tetrachords played by the *kitharōidoi* let there be constructed, first, the fourth from *nētē* to *paramesē* belonging to what is called the *tropoi*. Let this be ABCD, with A assigned to *nētē*. I say that what this contains is the genus of the tense chromatic that has been set out, and first that the ratio of A:B is 7:6, while that of B:D is 8:7. Those of BC and CD will be shown subsequently. Now each of AB and BD will be found to make a magnitude greater than a tone, that is, greater than the ratio 9:8, and the ratio of AD is 4:3; and no two ratios greater than 9:8 fill out the ratio 4:3 except 7:6 and 8:7, so that of the ratios AB and BD, one will be 7:6, the other 8:7. Next let there be taken the note H, equal in pitch to B, and let there be constructed upwards from it the tetrachord EFZH, similar to ABCD. Now A will be found to be higher than F (B and H being of equal pitch), and hence the ratio of AB is greater than that of FH, while it was laid down that the ratio of FH is the same as that of BD. Hence the ratio of AB is greater than that of BD, and hence that of AB will be 7:6, while that of BD will be 8:7. (42.10–43.8)

The construction is illustrated in Figure 11.01.

The analysis Ptolemy proposes for the attunement under investigation is set out in detail in I.16 and II.16. It is one of those used by *kitharōidoi*,

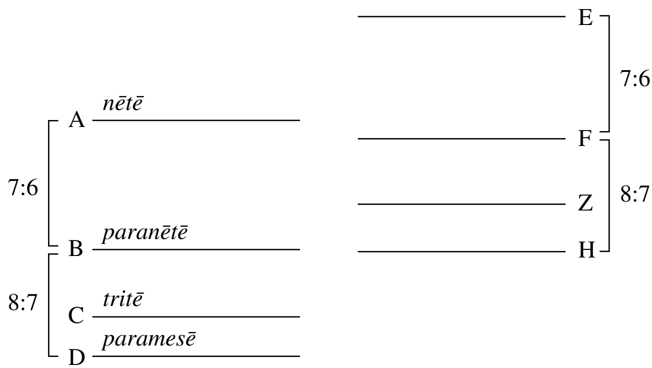


Fig. 11.01

singers to the *kithara* (who were generally professionals), the one given the name *tropoi*, presumably by the singers themselves. In our passage, Ptolemy is seeking to confirm his account of just part of its structure, his account, that is, of the relations between three of its notes in one of its tetrachords. According to that account, the ratio between its *nētē diezeugmenōn* and its *paranētē diezeugmenōn* is 7:6, and that between its *paranētē diezeugmenōn* and *paramesē* is 8:7.[8] How then does he go about showing that this is true?

The first step is to construct the tetrachord in question on the strings of an instrument, not on the basis of Ptolemy's analysis, but in such a way as to satisfy the musical ear. One could, for example, employ a *kitharōidos* to do the tuning himself, to ensure that it is correct by the standards of a practising professional. It need not even be done on one of Ptolemy's 'experimental' instruments. The conditions could be made still more realistic by using the instrument regularly employed by the musician himself. What matters, as we shall see, is that it be done in practice. Without that there will be no basis at all for an argument.

The second step in Ptolemy's exposition is a prediction: 'each of AB and BD will be found to make a magnitude greater than a tone'. The prediction can only mean that these facts will be discovered by ear, since we have been provided with nothing from which they would follow argumentatively. There are two ways in which the judgements might be made. One is simply to pay careful attention to the sounds of the intervals in question, leading to the conviction that each of them *sounds* larger than what we think of as a tone. But if this is reckoned too impressionistic and

[8] These names are given according to the 'thetic' system of nomenclature, 'by position'.

unreliable, there is an expedient to hand. We turn to one of the experimental instruments – the simple monochord would do – and construct accurately, according to its ratio (9:8), the interval of a tone, based on a pitch identical with one of the notes in the group we are studying. (Ptolemy evidently intends that this more reliable procedure be adopted; that is why he includes the ratio of the tone as one of the things that his argument assumes (42.8–10).) We might construct first, for instance, the interval of a tone upwards from the pitch of the *paramesē*. We would then compare the higher of the notes sounded on the monochord with the pitch of *paranētē* in the tuning of the *kitharōidos*, and if the former sounded lower in pitch than the latter, Ptolemy's prediction about interval BD would be confirmed. If it did not, the prediction would be straightforwardly refuted, and the whole demonstration would fail. We would then repeat the process for interval AB, constructing a tone upwards from the pitch of *paranētē*, and comparing the pitch reached through this interval with that of the note *nētē*. No very refined auditory acuteness is needed for this kind of comparison, where we are called on only to judge whether one pitch is higher than another. It corresponds to what Ptolemy says about comparing linear quantities by sight at 4.19–21.

Ptolemy now asserts that the whole interval AD is in the ratio 4:3. No argument is offered to support the claim, but none is needed. The tetrachord is agreed to be one that lies between fixed notes, and is bound to be a perfect fourth; and the ratio of the fourth was the other of the two things said to be assumed (42.8–10).

Next, Ptolemy introduces the mathematical observation that the only two (epimoric) ratios, each greater than 9:8, which will combine to give the ratio 4:3 are 7:6 and 8:7. Given that AD is 4:3, and that AB and BD are both greater than the 9:8 tone, it follows deductively that one of them is 7:6, the other 8:7. The supplementary word 'epimoric' is of course essential. Ptolemy takes it for granted that his procedures for dividing tetrachords, set out in I.15, are on the right lines, and specifically that the initial division of the fourth into two smaller intervals must assign epimoric ratios to each of them (33.27–34.4). This is the one 'theoretical commitment' involved in the present passage which Ptolemy does not explicitly identify in his opening remarks. I shall say something about its status shortly. But let us first complete the demonstration.

The last part of the project is to decide which of the two ratios, 7:6 and 8:7, belongs to which of the intervals AB and BD. The strategy is unproblematic. We are to construct by ear another example of the same tetrachord (or get our obliging professional to do it for us), one, that is, whose pattern of intervals still corresponds to what musicians mean by 'the tetrachord from *nētē diezeugmenēn* to *paramesē* in the attunement called

tropoi'; but we are to construct it in a different range of pitch. The pitch of its lowest note is to coincide with that of the *paranētē* of our original example. All we have to do now is to decide whether the interval FH in the second example is greater or smaller than AB in the first; and we do this simply by judging, by ear, which of the notes A and F is the higher in pitch. If Ptolemy's account of the attunement is correct, F will be lower than A. Hence FH is a smaller interval than AB, and since interval FH is the same size as BD in the original example, BD is smaller than AB. Hence it must be AB that has the larger ratio, 7:6, and BD that has the smaller, 8:7; and that was what Ptolemy set out to show. But if, at this final step, note F is judged to be higher than note A, it will be clear that Ptolemy's analysis was mistaken.

In the rest of the chapter Ptolemy works through a chain of similar operations to establish the adequacy of his analyses of all the attunements which he attributes to practising musicians in 1.16. I call the series a 'chain', because they are to a degree interdependent. That is, Ptolemy's method for confirming his account of some of the later specimens depends on aural comparisons between intervals they contain and intervals in attunements whose structure has already been confirmed. Hence if the tests show that his analyses of earlier attunements are at fault, the fact may put at risk his procedure for confirming others. They do not all stand or fall together, but the removal of certain bricks will destabilise a good deal. This interdependence, however, is the only feature of the later operations which distinguishes them in a methodologically significant way from the first.

What stands out most prominently in this important chapter is the absolute requirement that the procedures be conducted in practice. There are some purely argumentative steps, as we have seen; the clearest example is the one yielding the preliminary conclusion that the two ratios we are seeking must be 7:6 and 8:7, without deciding which is which. But the demonstration as a whole cannot work by argument alone. It hangs crucially on the ear's judgement, first that AB and BD are both greater than a tone, and secondly that the pitch of note A is higher than that of note F. Exactly comparable judgements are required in the cases examined later. Then either the whole chapter (four full pages of technically complex material, in Düring's edition) is the merest pretence, or else it records procedures which students of the science are seriously expected to go through in practice. At the very least it is an open invitation to them to do so, and it tells them in minute detail how Ptolemy's analyses could be refuted, if in fact they are wrong. In that case, whether anybody actually went through the whole series of operations or not, the presence of these procedural recipes in the text *by itself* puts Ptolemy's contentions

genuinely at risk. The procedures are unquestionably empirical, and if the results fail to match Ptolemy's predictions his analyses must be rejected. Here, it seems to me, we have a very strong case indeed for ridding the word 'tests' of its cautionary inverted commas. These are tests in the fullest sense; and one may reasonably guess that it was partly through procedures of the sorts described here that Ptolemy reached his diagnoses of this set of attunements in the first place.

There is one loose end, to which I said I would return. The assumption that both of the ratios involved in our case-study are epimoric is essential to the reasoning, and parallel assumptions are made repeatedly in the rest of the chapter. These assumptions are evidently not among the propositions tested here. They were given theoretical grounding in I.6 and I.15. But that by itself should not be enough, on Ptolemy's methodological principles, to qualify them as scientifically confirmed. They are high-level 'rational *hupotheseis*', and as such need to be brought to the judgement of the ear before they are finally accepted. (Thus the Pythagorean *hupotheseis* about the concords, it will be recalled, were not shown to be rationally unacceptable or incoherent; what made them untenable was their conflict with the evidence of the ear.) We must conclude, I think, that Ptolemy takes his own primary *hupotheseis* to have been given all the empirical warrant they require by the results of the tests, or 'demonstrations', referred to in the last paragraph of I.15, where he claimed that the ear would accept the perfection of all the tetrachordal divisions derived theoretically from the *hupotheseis* in that chapter.

I have already suggested that these demonstrations have problematic features (pp. 238–9 above). But let us suppose for a moment that they are methodologically flawless, and that they yield the results that Ptolemy claims for them. It is still not clear that they warrant the confident use of the *hupotheseis* in the present context. They show, at best, that the ear accepts all Ptolemy's theoretically derived divisions as perfectly formed from a musical point of view. Strictly speaking, that does not unambiguously confirm that the *hupotheseis* from which they were derived have any standing as principles of musical construction; the musical excellence of the results might be the merest coincidence. But that is a quibble. Given the restrictive nature of the *hupotheseis* and the complexity of the procedure involved in deriving the divisions from them, the coincidence would be an extraordinary fluke. It probably comes as close to genuine confirmation as can be reached by a procedure based on hypothesis, deductive derivation and empirical test.

The real problem is that even if every one of the derived divisions is musically well formed, there is nothing in the procedure to guarantee that all divisions which musical perception accepts as well formed must be

consistent with the *hupotheseis*. This would not matter if it had already been shown that all the divisions used by musicians belong to the set that Ptolemy has theoretically derived and tested. That this is so has indeed already been stated in the latter part of I.16. Each of the tetrachords in the attunements of musical practice is equated with one of those in the theoretically derived collection. We are apparently supposed to have done the relevant comparisons and found the match to be perfect. In that case all the intervals of the tetrachords under consideration do indeed conform to the *hupotheseis*.

But when Ptolemy introduces the passage we have been discussing, at the beginning of II.1, he shows every sign, as we have seen, of regarding his new procedure for confirming the analyses as completely independent of that suggested in I.16. In view of the assumption about epimoric intervals, it seems clear that the implication that these new tests can stand on their own is unwarranted. Empirical confirmation of the assumption lies, if anywhere, in the tests alluded to in I.15 and I.16, and without it Ptolemy's new procedure will get nowhere. There is evidently an opening here for critics who would wish to accuse Ptolemy of dishonesty. That is a possible diagnosis, but without further evidence it does not seem to me the likeliest. More probably, I suggest, Ptolemy is by now so used to operating with the presupposition that epimoric ratios have privileged status that he does not even notice the difficulty. There is no hint in I.15 that it is one of the propositions potentially put at risk by the empirical tests. By that stage it is only the fine details of his derivations and the divisions resulting from them that are occupying his attention.

One major phase of Ptolemy's investigation remains to be considered. It runs from II.12 to II.16. By the end of II.11 he has completed his theoretical discussion of the *tonoi*. Now, he tells us, it is time to bring the conclusions he has reached in their turn to the judgement of the ear; and he apparently proposes to present them in versions that incorporate his earlier analyses of all the different genera ('apparently', because the project will eventually be specified in a different and in some respects more limited way). In effect, it would seem, the whole body of his theoretical conclusions is to be brought simultaneously before the bar of musical perception.

Our remaining task, in the enterprise of displaying with complete clarity the agreement of reason with perception, is that of dividing up the harmonic *kanōn* – not just in one *tonos*, such as the unmodulated *systēma*, nor in one genus or two, following the practice of our predecessors, but in absolutely all the *tonoi* and each of the melodic genera, so that we may also include, set out jointly, all together, the positions that the notes have in common. (66.6–11)

The central task that must be undertaken if this programme is to be carried out is to present his analyses of the various systems in tabular

form, in such a guise that the experimenter can identify the points to which the bridge or bridges of his instruments must be moved to create the appropriate attunements. In practice, Ptolemy's presentation comes in two parts. In the first, he sets out once again his analysis of the division proper to each genus, exactly as he did in Book I, except that in Book II the divisions are extended from the single tetrachord to cover the span of a complete octave. Since we now have an account of the system of *tonoi* behind us, he can explain that they are presented here in the form they take in the central octave of the Dorian *tonos*, which is identical, as we have seen, to that of the 'unmodulated' *systēma*, the series in which dynamic and thetic designations of the notes coincide. This is done in II.14; and beside the tables of ratios representing his own analyses, he again sets out tables showing the corresponding divisions of his predecessors, Aristoxenus and Archytas, as in Book I, together with two newcomers, Didymus and Eratosthenes. He has explained in the previous chapter why he does this. 'In order to make readily available the contrast between our divisions of the genera and those that have previously been handed down – those, at any rate, which we have come across – we shall set out a partial comparison of them, in the middle *tonos*, the Dorian, to display in just that case the difference that there is' (69.8–12). The second phase of his presentation, where his generic divisions are fitted to the structures of *tonoi* other than the Dorian, is delayed until II.15.

The two collections of tables bring together a large amount of quantitative data, and the discriminations that the ear will be required to make when they are transferred to an instrument are complex and sometimes minute. It is not surprising, then, that before giving us the first tables, in II.14, he devotes the bulk of two chapters to further reflections on the merits and defects of some of the instruments that might be used. At the same time he takes the opportunity to review the attempts made by Didymus to develop an improved technique for playing the monochord. We looked at this material earlier (pp. 205–6), and need not revisit it here. What must concern us are the two passages which introduce the tables themselves, one at the end of II.13, the other at the beginning of II.15. In the first, Ptolemy explains in what terms the tabulated data will be expressed, and why that particular mode of presentation has been chosen (they are cast in rather different forms from the tetrachordal divisions of Book I); while the second states the purpose for which the tables of II.15 are intended, and accounts for certain limitations he will impose on them.

The beginning of the passage in II.13 is tolerably unproblematic.

In general, we have not undertaken our approach to the divisions in the same way as the older writers, dividing the whole length into the ratios indicated for each note, because of the laboriousness and difficulty of this sort of measurement.

Instead, on the *kanonion* that is placed up against the strings, we have begun by dividing the length cut off, from the highest limit of the sounding length to the mark there will be to indicate the lowest note, into divisions that are equal and proportionate in size. We have placed numbers against these, beginning from the highest limit, through however many parts may be involved, so that now we have got set out the numbers related in the ratios appropriate to each of the notes, starting from the common limit mentioned, we may always find it easy to bring the dividing points on the moveable bridges up against the positions indicated by the *kanonion.* (69.13–24)

This all sounds very sensible, and it accounts for the way in which these tables differ from the representations of the tetrachords in Book I. If the relation between each position of the bridge and its successor were again expressed merely as a ratio, the person setting up the attunements in practice would have a mass of calculations to perform in order to determine the points along the string where these successive bridge-positions should be located. Instead, we are to mark off a ruler, equal in length to the length of string that will give the lowest note to be used, into a number of equal segments (120 of them, as it turns out); and Ptolemy, for our comfort and convenience, will identify each bridge-position simply by a number representing a length of between 0 and 120 units, corresponding to a point some distance along the ruler. This is entirely legitimate, and will make the practical task of locating the bridges a great deal easier.

Ptolemy's next statement also reveals a practical or pragmatic approach to this phase of the operation.

Since it turns out that the numbers containing the differences shared by the genera run into tens of thousands, we have used the nearest sixtieth parts of complete whole units, down to the first sixtieths of a single unit, so that our comparisons are never in error by more than one sixtieth of one of the parts into which the *kanonion* is divided. (69.24–9)

This point, too, is essentially straightforward. If the distances along the ruler corresponding to the various bridge-positions are all to be expressed as multiples of the same unit of measurement, the unit will have to be very small indeed. It is a fraction of the ruler's whole length whose denominator, as Ptolemy puts it, 'runs into tens of thousands', and precise calibration of the measuring rod becomes practically impossible. Hence Ptolemy opts for a system of approximation. In choosing to express the relevant numbers to the nearest sixtieth of a unit he is no doubt influenced as much by his use of sexagesimals in astronomy as by any other consideration; but in principle nothing hangs on the choice. The important point is that the decision to use approximations makes sense only in the context of a procedure in which the ruler is really to be marked, and the bridges really placed in their positions. If nothing but mathematical exposition

were involved, not only would approximation be out of place; it would also be unnecessary to express all the distances in terms of the same fractional unit. Thus when both Düring's edition and my own published translation diverge from Ptolemy by giving exact, rather than approximated values in the tables, they can do so without using fractional denominators 'running into tens of thousands' by choosing different denominators for different cases. (In only two instances do the denominators reach four figures.) Düring presumably judged, as I did, that most modern readers are unlikely to want to use the tables to construct the attunements in practice; and it was then as easy to use exact values as it would have been to approximate to the nearest sixtieth. But in the practical context of setting bridges under strings, we need a consistently calibrated measuring rod; and it would be absurd to demand a more precise identification of each position than one approximated to the nearest sixtieth of one of Ptolemy's 120 units. Unless the units are made very large indeed, and the string correspondingly long, a variation of less than one sixtieth of a unit could hardly be made with any accuracy, and will in any case make no musically distinguishable difference.

The final paragraph of II.13 describes the basic topography of each table.

> In order that the distance covered by the fourth below the disjunction may span thirty parts, the number proposed by Aristoxenus, and in order that when we take his divisions in the larger context we may still understand the segment consisting of a tetrachord through the same numbers, we have posited that the length from the common limit to the lowest note of the octave set out consists of 120 segments, and the note higher than this by a fourth is 90, in epitritic ratio [4:3], so that the note a fifth higher than the lowest is 80, on the basis of hemiolic ratio [3:2], and the highest note of the octave is 60, in duple ratio [2:1]. The intermediate, moveable notes take their numbers in accordance with the ratios of each genus. (69.29–70.4)

Now the numbers mentioned, 120 for the lowest note, 60 for the note an octave above it, 90 and 80 for the notes a fourth above the lowest and a fourth below the highest, are neat and simple and fit the appropriate ratios; and they mark the principal pivots, the fixed notes, in the central octave of the standard, unmodulated series. But the reference to Aristoxenus is worse than careless. The problems associated with it are significant enough to deserve a discussion of their own.

It is true that Aristoxenus' divisions can conveniently be represented in terms that assign 30 units to the span of the fourth, as between 90 and 120. But Aristoxenian 'units' are not lengths along a string. They represent small *intervals*, each equivalent to one twelfth of a tone; and the same interval will of course not always be generated by a movement of the bridge through the same distance along the measuring rod. To move upwards through a tone, for example, we must shift the bridge so that the

new length is eight ninths of the old one. Hence we move upward through a tone by shifting the bridge from the point marked '90' to the point marked '80', a distance of 10 units. But to move through the same interval from the note given when the bridge is standing, for instance, at the point marked '117', we must move it to give a sounding length that is eight ninths of 117 units, that is, to the point marked '104'. Here the movement through a tone requires us to shift the bridge through a distance of 13 units. To equate Aristoxenus' thirtieths of a perfect fourth with the thirty units of distance along the string between 120 and 90 is therefore absurd.

When we look at the tables themselves to see how Ptolemy has in fact represented the Aristoxenian divisions, it is at once obvious that things have gone badly wrong. He cannot in practice assign equal distances to equal intervals throughout the span of the octave, since the numbers would not add up. Nevertheless he insists, perversely, on doing the best he can. Thus in the lower tetrachord each Aristoxenian tone is represented by a move through 12 units, each half-tone by a move through 6, and so on. Since the notes bounding the various tones and fractions of tones in any given division are at different pitches, and correspond to different string-lengths, that is bad enough. But in the upper tetrachord, the interval of a fourth between the points marked '80' and '60' is not occupied by the postulated 30 units. Hence in this part of the system, quite without explanation, Ptolemy assigns each Aristoxenian tone a value of 8 units instead of 12. Equally arbitrarily, from this perspective, the interval of a tone in the middle of the octave is assigned the 'distance' of 10 units between 90 and 80. The interval sounded in this case will indeed be a tone (which it will not be in any of the others), but that is because the two lengths are in the ratio 9:8, not because the tone 'is' a distance of 10 or any other number of units.

Ptolemy's representation of Aristoxenus' divisions is therefore wholly misguided. In fact, in the absence of a system of logarithmic 'cents', the mathematical resources at his disposal could not have given him a procedure for marking them out as his project requires. To do that, he would first have had to convert them into systems of ratios, comparable to those specifying his own attunements and those of the other theorists he discusses. Divisions expressed in this form can readily and properly be translated into ratios between lengths of string, and the appropriate positions for the bridges located on the measuring rod. But Ptolemy had no means for converting Aristoxenus' halves and quarters of a tone, and so on, accurately into the terminology of ratios. If a tone is in the ratio 9:8, there is no ratio of integers which corresponds to a half or a quarter of this interval.[9]

[9] This mathematical truth is stated in its general form by Ptolemy at 24.10–11 and 30.7–9. For a proof see [Eucl.] *Sect. can.* props. 3 and 16.

Ptolemy's confusion about the different mode of representation used by the Aristoxenians is strongly suggested also by a polemical passage at 20.22–21.8, which we glanced at earlier (pp. 96–100 above). Given the sophistication of his other mathematical operations, it is hard to understand how he came to make so elementary a mistake. But it seems likely that the absurdities in the tables of II.14, and in the preface to them in II.13 which we have been discussing, are not originally of Ptolemy's own making. It is a striking fact that the numbers attached to Eratosthenes' divisions in II.14, to indicate the points on the ruler against which the bridges are to be placed, are exactly the same in the enharmonic and the chromatic as are those assigned, by the arbitrary procedures I have described, to the divisions of Aristoxenus. (Their diatonics are slightly different, but the reasons for this are easily grasped.) The repetition can hardly be a coincidence; and it seems almost beyond doubt that Eratosthenes designed his divisions as 'translations' of Aristoxenian divisions into the terminology of ratios. There seems to be no other way, short of the hypothesis of textual corruption, to explain the reappearance of these sets of numbers. But in that case the misleading representation of Aristoxenus' systems must also be his. Ptolemy seems simply to have taken it over, without pausing to inspect its credentials.

If that view of the matter is right, he is clearly guilty of failing to exercise rudimentary caution of a sort that was well within his mathematical competence. Alternative readings of the passage would be still less flattering. If Ptolemy were himself responsible for introducing the confusion, he must have been either extraordinarily careless or plain dishonest, hoping that his readers would fail to notice that the procedure is the merest nonsense. Given the Eratosthenean precedent I incline to the first interpretation; and we could plausibly add the suggestion that Ptolemy was so uninterested in the Aristoxenian divisions and so contemptuous of the style of analysis that gave rise to them, that an investigation of the basis of Eratosthenes' putative 'translations' never struck him as an enterprise worth undertaking.

I have spent some time on this short passage, and its consequences for the tables in II.14, precisely because it shows Ptolemy at his worst. Unless the tables allow us to bring the systems under scrutiny accurately to the judgement of the ear, they are worthless and may be positively misleading. In the case of Aristoxenus' divisions they fail disastrously. For reasons we have seen, Ptolemy was not really in a position to do the job properly in any case; but that does not amount to a compelling defence, and in fact he seems hardly to have tried. Fortunately, however, none of the difficulties associated with Aristoxenian divisions affects any of the others. Since all of them were originally expressed as systems of ratios, all can be treated as

specifying ratios of string lengths. Ptolemy's way of marking out the ruler, with its method of approximation, gives a thoroughly workable procedure for transferring them undistorted onto the strings of an instrument. I see no reason to doubt, and have suggested good reasons for believing, that they were seriously meant for that purpose. If they are inadequate to the task of bringing Aristoxenus' divisions to the ear in the manner of the others, that is a defect, no doubt, but not one that need undermine the conviction that these operations were intended to be carried out in practice.

The paragraph at the beginning of II.15 explains the purpose of the large collection of tables in that chapter. In doing so it strikes an unexpected note. In II.12 Ptolemy had said that his remaining task was 'that of dividing the harmonic *kanōn* . . . in absolutely all the *tonoi* and in each of the melodic genera' (66.6–10). What we expect, then, at the climax of the work in II.15, is a set of tables giving numerical values to the positions of the bridges for every one of the theoretically derived generic divisions in each of the seven *tonoi*. There might be seven tables, one for each *tonos*, probably over the span of an octave. Each would have eight separate columns of figures, one for each genus as it appears in that octave of that *tonos*, corresponding to Ptolemy's one enharmonic division, his two chromatics and his five diatonics.

In fact Ptolemy provides fourteen tables, two for each of the *tonoi*. One member of each pair shows the structure of the central octave, from *nētē diezeugmenōn* ('by position') down to *hypatē mesōn*, the other the structure of the octave running down to *mesē* from the highest note of the system, or down from *mesē* to the bottom (the forms of these octaves are always identical). But this is not the main point at which our expectations fail to square with the facts. Ptolemy's fourteen tables have only five columns each, and only one of them contains the numbers proper to a single genus. Three of the generic divisions presented in II.14, enharmonic, soft chromatic and even diatonic, do not appear in the tables of II.15 at all.

The reasons for all this are explained in the first paragraph of the chapter; the first sentence refers back to the tables of II.14.

It is for one purpose only, as we said [69.8–12], that we have started by setting out the shape of these divisions, that of assessing the differences proper to the genera. To accomplish our remaining task, that of expounding the practice of modulations of the octave, we took in the same way the constitutive numbers for each of the seven *tonoi*, those that accommodate the familiar genera of melody. We did this, further, in the way that each of them is naturally linked together throughout its whole series: we took, that is, for the one that can be sung just by itself, the numbers divided up for ratios within the same genus, but for those that are sung in part, in a special combination with the one mentioned (unless one is prepared to

use force), we took from the combination of ratios the numbers attuned to the positions appropriate to the mixture, in order to disguise the fact that we too have gone beyond the limits of what we ought to do, since we have already busied ourselves too much with the divisions of unfamiliar genera. (74.4–15)

It appears, then, that the dénouement towards which the entire complex of argument, observation, calculation and construction in Books I and II has been leading is not quite what we might have taken it to be. It is not a quantitative representation of every structure generated by Ptolemy's theory, enabling the complete collection to be submitted to the judgement of the ear. In saying that we have already 'gone beyond the limits of what we ought to do', and 'busied ourselves too much with the divisions of unfamiliar genera', Ptolemy is not just admitting that there are special difficulties in assessing these divisions aesthetically, a point that I argued above (pp. 238–9). He is saying, in effect, that such divisions are irrelevant to the proper concerns of harmonic science.

The tables do not even represent all those generic divisions which Ptolemy had treated as recognisable and agreeable to the ear at the beginning of I.16 (38.2–6). What they give is only a selection from that repertoire, combined in such a way as to permit Ptolemy's analyses of the patterns of attunement used by practising musicians, and nothing else, to be tested against the listener's aesthetic sensibility. The point deserves special emphasis. For one thing, this feature of Ptolemy's programme is unparalleled elsewhere. No other Greek author sets himself a goal of this sort, or restricts the task of harmonic science in a comparable way. The only other theorists who explicitly recognise a distinction between what theory requires and what human perception accepts belong to the Pythagorean and Platonist traditions, and they invariably treat the former as the only appropriate subject for study.

Secondly, it shows that Ptolemy has taken as his objective the analysis of something pervasively present and widely admired as a thing of beauty in the familiar world. His subject is not melody as it might be, something to be constructed in the study or even in the laboratory, but as it is, out there in the concert-halls and theatres. It is a set of phenomena already present in human experience, awaiting scientific analysis, rather than some other, theoretically perfect set of structures that ingenious minds might devise, even if a listener might be persuaded to agree that the latter, too, for all their 'unfamiliarity', make an elegant impression on the ear. The task of the science is to analyse a familiar element in the world as we encounter it, something that is empirically 'given', and to show that its manifest but puzzling excellences can be understood as instantiations of intelligible mathematical form.

Finally, a project of this sort makes its conclusions quite radically vul-

nerable to empirical falsification. This is not just because the structures presented to our perceptual judgement purport to be ones that will satisfy the musical ear, and the conclusions will be put in doubt if we find that they do not. They are identified explicitly with particular, named systems of attunement that were in regular contemporary use. Ptolemy meticulously picks out the correspondences between tabulated sets of numbers and the named attunements they are designed to represent at the beginning of II.16. Hence the issue to be put to the test is no longer the potentially unreliable one of whether this or that division pleases the hearer. It is simply whether or not the division matches the pattern of intervals to which an accredited musician tunes his instrument. That is a question to which the ear can give a much more nearly conclusive answer. Ptolemy has provided his readers with all the data they need to conduct a serious test. He has told them in great detail how to do it, and the test is as clear-cut and objective as such a thing could be. He has quite genuinely presented his conclusions for trial by empirical jury.

Two loose ends are left to be tidied up. On the account I have offered, some of the columns of figures in the tables of II.15 would appear to be otiose. The named attunements adopted by players of the *lyra* use only two of the genera (80.8–11), and those used by *kithara*-players use only four of the seven *tonoi* (80.11–18). There seems to be no role for a good many of the divisions tabulated (that of the mixture of soft diatonic with tonic diatonic in the Mixolydian *tonos*, to take just one example).

Part of the answer is that the *lyra* tunings, though restricted to two genera only, are apparently used indifferently in every *tonos* (80.8–11). But that does not account for cases like the one mentioned above as an example, where the mixture of genera is itself alien to the attunements of that instrument. The other part of the answer must be that these divisions are needed to accommodate modulations of genus or of *tonos* in compositions for the *kithara*. These need not be modulations to another named attunement, only to a structure admissible as a transitory variant on the one from which the piece began. To take a simple example, consider the manoeuvre which had traditionally been thought of as shifting from the use, in a given *tonos*, of the tetrachord *diezeugmenōn* above dynamic *mesē*, to that of the tetrachord *synēmmenōn*. In II.6, as we saw earlier (pp. 168–73 above), Ptolemy analyses this change as a temporary modulation to the *tonos* lying a fourth above the original one. To accommodate it, therefore, we need access to a *tonos* different from the one in which the composition begins, which (we may fairly assume) will be that appropriate to one of the named attunements. Hence, to pick up the example mentioned above, though the only *tonos* associated, in the named attunements, with the mixture of tonic diatonic and soft diatonic is the Dorian, that mixture

must also be available in the *tonos* a fourth higher, the Mixolydian, if this modulation is to be accommodated. In order to make room for all the modulating possibilities of performing practice, each of the 'practical' combinations of genera must in fact be capable of being transported, directly or through a series of intermediate steps, into any of the *tonoi*. The additional columns in Ptolemy's tables are therefore not redundant. They allow for the option of testing his analyses against the attunements of practical music-making in all their modulated forms, as well as in their original guise.

It may seem curious, finally, that I have described II.15 as the climax of the work, and have drawn substantial conclusions from that view of the matter. It comes, after all, only two thirds of the way through the treatise. But this objection is readily answered. Once Ptolemy has matched the named attunements with corresponding divisions in the tables, at the beginning of II.16, he devotes the rest of the chapter to suggestions about various practical modifications to his experimental instruments. The first two chapters of Book III, similarly, are concerned exclusively with ways in which the design and use of these instruments can be refined for the purpose of accurate testing. (We have reviewed all this material above, pp. 211–26.) The task of these passages is thus to improve the technical equipment needed for an empirical assessment of conclusions already theoretically drawn; and we have seen that here too it is the named attunements of musical practice that are the principal focus of attention (see especially pp. 220–22 above). These chapters are therefore best interpreted as a pendant to the tabulated constructions of II.15.

Book III contains a further fourteen chapters. But at the beginning of III.3, Ptolemy announces that the task he set himself at the outset has now been fully accomplished.

> It seems to me, then, that we have demonstrated adequately and in several ways that the nature of attunement possesses its own proper ratios right down to the melodics, and that we have shown which ratio belongs to each of them, in such a way that those who strive eagerly to master both the rational grounds of the principles laid down and their assessment in practice – that is, the methods of using the *kanon* that we have expounded – can be in no doubt that they conform, throughout all the species, to what we accept on the basis of the senses. (91.22–92.1)

There is then no more for harmonic science, as such, to do; and from that point of view, II.15 was indeed the culmination of the enquiry. The remainder of the work shifts to a wider perspective, to survey the whole territory within which this science and its subject-matter belong.

12 Harmonics in a wider perspective

Since the opening of III.3 declares that the programme of harmonic science has now been completed, the rest of the work really falls outside the scope of this methodological study. I shall consider one part of it, the introductory section, in a little detail, and offer only a brief sketch of the contents of the rest. This will be enough, I think, to give us some purchase on Ptolemy's conception of the place of harmonics among the sciences, and its role in equipping us to interpret and engage with the universe we inhabit. We shall then be in a better position to understand why a scientist of Ptolemy's stature should have found this apparently small and insignificant corner of the Greek intellectual tradition worth the meticulous attention he has given it.

After drawing a line under the completed business of the science of harmonics, III.3 proceeds as follows.

> Since it is natural for a person who reflects on these matters to be immediately filled with wonder – if he wonders also at other things of beauty – at the extreme rationality of the *harmonikē dunamis*, and at the way it finds and creates with perfect accuracy the differences between the forms that belong to it, and since it is natural also for him to desire, through some divine passion, to behold, as it were, the class to which it belongs, and to know with what other things it is conjoined among those included in this world-order, we shall try, in a summary way, so far as it is possible, to investigate also this remaining part of the study we have undertaken, to display the greatness of this kind of power. (92.1–8)

This ponderously eloquent paragraph, and its sequel in the rest of the chapter, are unintelligible unless we can decide what they are about. It is the *harmonikē dunamis*; but what is that? If we translate it[1] as 'the power of *harmonia*', or 'the power of attunement', we shall apparently be investing attunement itself with independent reality and influence. Since it is plainly not a physical entity, we shall probably be led to think of it as something allegedly belonging to a metaphysical order, beyond the natural realm. When we find that it is treated as a principle determining

[1] As I did in *GMW2*, p. 371.

forms of movement, we shall be tempted to interpret it as something like a mysterious cosmic force, imposing its activity in unexplained ways on the contents and processes of the universe. There would be ample precedents; for early and well known examples we need only turn to the fragments of Philolaus, and before him to Heraclitus and Empedocles.

But this reading is almost certainly wrong. The expression *dunamis harmonikē* (the altered word-order is not significant) has already appeared in the *Harmonics*, in the very first line of the treatise. In that context, it is 'that which grasps the distinctions related to high and low pitch in sounds' (3.1–2). It is this *dunamis* whose 'criteria' are hearing and reason; and it is very clearly a power or capacity which we ourselves possess, whether as scientists or as musicians or simply as human beings. It is, in fact, the capacity which Ptolemy has been exercising throughout his investigation. The final part of the treatise is linked to the preceding material, then, in the role of an enquiry into the nature of the *dunamis* which has made that investigation possible; and the fact is signalled by the reappearance of the expression with which it was first introduced. The question which is raised in the passage I have quoted is how this capacity is related to others ('the class to which it belongs'), and to other subject matters ('other things . . . among those included in this world-order'). There will turn out to be rather more to it than that; but at the present stage the question is intelligible and sensible, and no alarming metaphysical phantoms need to be summoned up to help us interpret it.

Ptolemy begins his answer to the question with a classification of 'principles' (*archai*), closely modelled on a well known Aristotelian pattern.[2] These are 'matter and movement and form, matter corresponding to what underlies a thing and what it comes from, movement to the cause and agency, and form to the end and purpose' (92.9–11). These *archai* are most simply understood as distinct kinds of factor on which we call when seeking to explain the coming into being, characteristics or behaviour of any natural thing. Some aspects of what a Yellow-eyed Penguin, for example, is and does, and how it comes into existence, can be accounted for by saying '. . . because it is made of such and such materials'. Others require reference to the action of some agent or agents, notably its parents. Others again are to be understood as manifestations of the activity of a thing which is developing towards, or is already actualising, the form whose realisation is the 'end and purpose' of things of that specific kind.

The power we are considering, which is now called simply *harmonia*, is not, Ptolemy says, to be conceived as the matter which is moulded to

[2] See e.g. Ar. *Phys.* II.3.

produce something, 'for it is something active, not passive' (92.12–13). Nor is it the end or purpose, that which constitutes an actualisation of completed form. What count as 'ends' in connection with *harmonia* are such things as 'good melody, good rhythm, good order and beauty'; and these are not identical with *harmonia* or the *dunamis harmonikē*, but are things brought into being through its agency. This *dunamis*, then, is to be understood as 'the cause, which imposes the appropriate form on the underlying matter' (93.13–16). Despite the high level of abstraction, this passage again is readily understood if we identify the *dunamis harmonikē* with a kind of power or capacity which we ourselves possess, in so far as we are capable of bringing certain kinds of 'material' into good harmonic order. It is broadly analogous to our capacity to mould sounds into significant speech, or to conduct mathematical calculations; and it is no more (though no doubt also no less) ontologically puzzling than they are.

A second classification follows. There are three principal kinds of agency or cause. One is that which brings new things into being; the second does not bring anything into being, but organises for the good that which is already present; the third is the cause of the existence of things that are both good and eternal. Of these the first is identified with nature and the third with God, and the activity of *harmonia* does not correspond to either. It belongs to the second category, the province of reason (*logos*), whose role is to produce good order in materials already available to it (92.14–24). The capacity we are investigating, then, is a form of reason or a mode of its application.

In a third and final classification, Ptolemy distinguishes three aspects or manifestations of reason, conceived as a 'cause' constituting our capacity to do things of certain kinds. It manifests itself as intelligence or understanding (*nous*), as constructive expertise or skill (*technē*), and as habit or disposition (*ethos*) – that is, as the tendency to do things as reason would dictate, but without taking deliberate thought about them (92.24–6). These aspects of reason in general are displayed also in the special case of *harmonia*, or 'harmonic reason' (this third designation of the subject appears first at 92.27–9).

For reason, considered in general and without qualification, is productive of order and proportion, while harmonic reason, in particular, is productive of them in the class of what is heard, just as is imagistic [*phantastikos*] reason in the class of what is seen, and critical reason in that of what is thought. It makes correct the ordering that exists among things heard, to which we give the special name *emmeleia* ['melodiousness'], through the theoretical discovery of proportions by means of intelligence, through their exhibition in handicraft [*cheirourgikē endeixis*] by means of skill, and through the *empeiria* of following them by means of habit. (92.27–93.4)

The broad sense of this passage is not hard to grasp, but there are interesting implications hidden in the details. The 'theoretical discovery of proportions' is most straightforwardly understood as the work of the harmonic scientist, or at least as the phase of it that proceeds through 'rational *hupotheseis*', mathematical derivations and abstract argument. Their 'exhibition in handicraft by means of skill' might also be an aspect of the scientist's activity, referring to his manipulation of instruments when he brings his conclusions to the judgement of the ear. I am confident that this is part of what Ptolemy means. But in view of the earlier statement that this *dunamis* is the one responsible for all 'good melody' and 'good rhythm' (92.14), the expression must also refer to the activity of musicians themselves, as they go about their music-making. This interpretation is strengthened by the third manifestation of harmonic reason, 'the *empeiria* of following them by means of habit' where *empeiria* designates an ability born of repeated experience. This ability, it seems to me, must be that of the experienced listener to music, who – even if he lacks the levels of understanding and skill required to grasp the proper schemes of formal proportions or to exhibit them in practice – can at least 'follow' music and perceive its complex excellences under their aesthetic aspect.[3] It is an ability that the harmonic scientist certainly requires, at least in some degree, as a precondition of his aural assessment of the credentials of his constructions. But this use of the ability seems secondary. Its primary role is in enabling the audience at a musical performance to appreciate what they hear. Even if this ranking of 'primary' and 'secondary' uses were challenged, it would still be the case that the ability exercised is the same in both cases. The credentials of the scientist's auditory assessments depend, in fact, on their being so.

The upshot is that the scientist who studies harmonics, the musician who performs and the listener in the audience are all exercising the same faculty of 'harmonic reason', though in different ways. There is no unbridgeable gap between the activities of the musician and those of the scientist, and the scientist is not imposing on his subject matter, music, principles that are alien to its practice and its aesthetic appreciation.[4] In proceeding to the 'theoretical discovery of proportions' he certainly seems to be extending the use of harmonic reason beyond anything that

[3] The noun used here for the business of 'following', *parakolouthēsis*, seems to have been part of the semi-technical vocabulary of the subject. See for instance Aristox. *El. harm.* 38.27–39.3 (using the cognate verb *parakolouthein*), [Plut.] *De mus.* 35, especially 1144c (where the noun *parakolouthēsis* itself appears).

[4] That was the accusation levelled by Aristoxenus against the mathematical style of harmonic theory, *El. harm.* 32.18–28.

would normally be expected of musicians themselves.[5] Even here, however, the activities of scientist and musician run parallel to one another. The business of composing rather than performing music does not seem to fit comfortably under any of Ptolemy's three modes of harmonic reason. It is neither a process of 'following' music as it is presented, nor one of 'exhibiting it in handicraft'; nor is it the 'theoretical discovery of proportions'. What the composer contrives is a pattern of notes and intervals imagined under the guise in which they strike the ear, as perceptible modifications and interrelations of sounds, not as systems of ratios intelligible to the mind. But we saw at the outset (pp. 17–18 above) that the relevant perceptible modifications and patterns of sound are identical with the formally ordered quantities and ratios of quantities that the scientist studies. In perceiving or imagining the aesthetic merits of his composition, as he constructs it, the musician is exercising the same capacity to identify fine and beautiful sets of relations as is the scientist when he appreciates the mathematical excellence of the corresponding forms, though they are approaching them in different ways and under different aspects. In fact, since the scientist (in the ideal case, at least) is using the aesthetic sensibilities of his hearing in partnership with his mathematical intelligence, he is deploying a repertoire which itself includes the contents of the composer's own aesthetic tool-kit. The scientist's is simply more complete.

In the next sentence, Ptolemy moves out again from the restricted sphere of harmonic reason to the wider faculty within which it is contained.

> When we consider this – that reason in general also discovers what is good, establishes in practice what it has understood, and brings the underlying material into conformity with this by habituation – it is to be expected that the science that embraces all the species of science that rely on reason, which has the special name 'mathematics', is not limited solely by a theoretical grasp of beautiful things, as some people would suppose, but includes at the same time the exhibition and *meletē* of them, which arise out of *parakolouthēsis* ['following'] itself. (93.4–10)

Mathematical reason in general, then, like harmonic reason, manifests itself in three ways, of which theoretical understanding is only one. The second and third are both said to arise 'from *parakolouthēsis* itself', where *parakolouthēsis* is the same word that was used for 'following' a musical sequence, a few lines earlier. The second is the practical construction or 'exhibition' of beautiful things. The third is *meletē*, a term which can refer

[5] But there are writers who express the view that musicians, and especially composers, must also be well versed in the technicalities of musical theory, though they need other accomplishments as well. See particularly Plato *Laws* 670b-e, [Plut.] *De mus.* chs. 33–6, especially 1143b–e.

to the practice or exercise of something, or to the concern and attention with which it is regarded. If the analogy with the modes of harmonic reason is to be maintained, the sense here is probably the latter. From the experience of 'following' patterns of beauty we acquire the disposition to attend to them and value them.

Another important point made in this passage concerns the items that form the subject matter of the mathematical sciences. They are not specifically mathematical entities, such as numbers or abstract lines and planes, nor are they 'things in general', as a mathematical cosmologist might claim. They are 'things of beauty'. That is, they include such things as perceptibly beautiful melodies and melodic relations, and the other subjects of mathematics are analogous to these. They are items in the world accessible to our senses, but only those among them that display themselves to our senses as beautiful. This leads to a remarkable conclusion. Mathematics is not the study of all quantities and all quantitative relations indiscriminately. It is the science of beauty. Its task, at the theoretical level, is to interpret, in terms of 'rationally' or mathematically intelligible form, the features, movements or states which, when they are present in perceptible phenomena, constitute their aesthetic excellence. No doubt other conditions of such phenomena could also be represented in quantitative terms – the ratio between the pitches of a given pair of unmusically related sounds, for example. But this will provide useful knowledge only of a negative sort, the knowledge that their aesthetic inadequacy reflects their failure to instantiate the system of ratios or proportions that is determined by appropriate rational principles.

Those of our senses through which we are able to perceive some things as beautiful are therefore involved in an intimate collaboration with mathematical reason. The capacities for the creation and appreciation of aesthetically beautiful things which we exercise through those senses are in fact, as we have seen, modes in which mathematical reason itself is manifested. Ptolemy goes on to assert that only two of the senses, sight and hearing, are capable of such discriminations. 'This sort of power employs as its instruments and servants the highest and most marvellous of the senses, sight and hearing, which, of all the senses, are most closely tied to the ruling principle, and which are the only senses that assess their objects not only by the standard of pleasure but also, much more importantly, by that of beauty' (93.11–14). To distinguish the beautiful from the ugly, then, is not at all the same as distinguishing the pleasant from the unpleasant. Ptolemy accepts, of course, that smells and tastes and things felt can strike us as 'agreeable or disagreeable' (93.20). 'But no one would classify the beautiful or the ugly as belonging to things touched or tasted or smelled, but only to things seen and things heard, such as shape and

melody, or the movements of the heavenly bodies, or human actions' (93.20–23).

The distinction between the beautiful and the merely pleasant is at least as old as Plato, as is the tendency to associate beauty with rationally intelligible form, pleasure with mere titillation of bodily parts. Nor is there anything unusual in Ptolemy's attribution of privileged status to sight and hearing. It is interesting, however, that the distinction between the classes of sense faculty does not correspond directly to a distinction between those that can make quantitative discriminations, or discriminations grounded in quantitative differences, and those that cannot. To take only the most obvious case, it is neither through sight nor through hearing that we perceive one object as heavier than another. If we hold in one hand an object weighing twelve ounces and in the other an object weighing eight ounces, their weights are related in the ratio 3:2, the ratio, in music, of the aesthetically satisfying perfect fifth. The fact that we do not assess relations between weights as perceptually beautiful cannot then be due to the absence of appropriate relationships to be appreciated in that domain. It must reflect some inadequacy in the relevant sense faculty itself. Since beauty is the manifestation to the senses of that which reason understands as perfect in form, the senses to which beauty is undetectable lack sensitivity, which sight and hearing possess, to those distinctions which, from a rational point of view, are the most significant. They cannot work in cooperation with mathematical reason as its 'instruments and servants'.

Sight and hearing cooperate not only with reason, but with one another. 'These, alone among the senses, give assistance with one another's impressions in many ways through the agency of the rational part of the soul, just as if they were really sisters' (93.23–94.1). After an elaborate (but perhaps not entirely apposite) series of observations alleged to support this thesis, Ptolemy continues: 'It is therefore not just by each one's grasping what is proper to it, but also by their working together in some way to learn and understand the things that are completed according to the appropriate ratio, that these senses themselves, and the most rational of the sciences that depend on them, penetrate progressively into what is beautiful and what is useful' (94.9–13).

The substantial point to be gleaned from this underlies the whole pattern of thought in this chapter. In cooperating with one another and with reason, sight and hearing are pursuing the same quarry. They assist one another in so far as they provide reason with clues, of different but mutually supporting kinds, to the identity of the object, perceptible beauty, that it is called on to analyse. They submit to the consideration of reason different sets of 'distinctions that they have grasped in rough

outline' (3.12–13). From that starting point reason proceeds to seek the mathematical principles to which all these perceptible manifestations of beauty conform in their formal aspect, to derive from them what it construes as precise formal counterparts of the relations to which the senses have alerted it, and to return them to the senses for them to assess. Seen in this light, the mathematical sciences have a single objective, the analysis and understanding of the formal basis of beauty, and the senses of sight and hearing, different though they are, are allies in that single but very complex quest.

In the closing sentences of the chapter Ptolemy names the principal mathematical sciences with which sight and hearing cooperate, and sketches the relationships between the sciences and their various accessories, through a rather charming development of a metaphor pioneered by Archytas and Plato.[6]

> Related to sight, and to the movements in place of the things that are only seen – that is, the heavenly bodies – is *astronomia*; related to hearing and to the movements in place, once again, of the things that are only heard – that is, sounds – is harmonics. They employ both arithmetic and geometry, as instruments of indisputable authority, to discover the quantity and quality of the primary movements; and they are as it were cousins, born of the sisters, sight and hearing, and brought up by arithmetic and geometry as children most closely related in their stock. (94.13–20)

Harmonics and *astronomia*,[7] then, are offspring of hearing and sight, developed into their full stature through the instruction of arithmetic and geometry. They provide rational understanding of the patterns of movement perceived by the senses, rightly but vaguely, as manifestations of beauty. We should notice that arithmetic and geometry are not themselves classed among the mathematical sciences; they are instruments that these sciences use, or tutors by which they are trained. Their status seems to be analogous to the one attributed by Aristotle to logic, in relation to philosophy proper; it is an *organon*, an instrument of philosophical reflection rather than a branch of philosophy in its own right. Ptolemy identifies the mathematical sciences by reference to the specific subjects, presented to them initially through sight and hearing, which they are called upon to explore. Geometry and arithmetic are not in this sense mathematical sciences because, like logic, they are not studies that

[6] See the opening of Archytas fr. 1, Plato *Rep.* 530d. On the question of the authenticity of the relevant words in Archytas (some of them appear only in the version given by Nicomachus, not in that of Porphyry), see *GMW2* pp. 39–40 nn.42–4, with the references given there.

[7] I have transliterated this word rather than translating it because of a certain ambiguity in its application. The issue is discussed below, in the last two paragraphs of the chapter.

provide understanding of any specific subject in the realm of perceptible reality, but are tools to be used by any and all of the sciences that deal with quantity.

Now the generalising tendency of III.3 may lead us to suspect that elegant patterns of musical sounds and celestial movements cannot be the only proper objects of mathematical science, even though they are the only ones mentioned by Ptolemy in his closing sentences. He himself has already mentioned 'human actions' (93.23) as included within the category of things to which beauty and ugliness belong. We may suspect also, given the close kinship indicated in this chapter between the senses capable of perceiving beauty, that the very same systems of mathematical ratios that are found, in the main part of the *Harmonics*, to correspond to well formed musical structures, will also be found to underlie significant relationships in other perceptual domains.

Both these suspicions are amply confirmed in III.4. All living things in nature, we are told, in so far as they are healthy and flourishing, are so in virtue of their maintenance of the proper harmonic ratios in their movements and among the elements from which they are made (95.4–6, 13–16). But these ratios govern most completely 'those that share in a more complete and rational nature' (95.6–7). 'In these alone can it [the *dunamis harmonikē*] be revealed as preserving fully and clearly, to the highest degree possible, the likeness of the ratios that create appropriateness and attunement in the various different species' (95.8–10). It is this power, or form of reason, that is responsible for the ordering of the movements of such beings, 'as among divine things the movements of the heavenly bodies, and among mortal things those of human souls, most particularly, since it is only to each of these that there belong not only the primary and complete sort of movement, that in respect of place, but also the characteristic of being rational' (95.21–4). The activities of stars and souls display 'the pattern of organisation that is based on the harmonic ratios of the notes' (95.25–6).

These programmatic remarks serve as an introduction to the remainder of Book III, which deals first with souls, then with stars. In III.5–6 Ptolemy sketches a set of proposed correlations between musical structures on the one hand and elements of the soul, and its virtues, on the other. III.7 outlines an intriguing series of correspondences between psychological changes that occur at various crises in our lives, and specific forms of melodic modulation. The remaining chapters (III.8–16) provide analyses, grounded in harmonic science, of significant pathways among the courses of the celestial bodies, and notable geometrical configurations of their positions. Some of these configurations and patterns of movement, together with their harmonic counterparts, are dissected in impressive detail.

I shall not succumb to the allure of this heady material so far as to attempt a commentary. The chapters on the soul and the virtues, rewarding though they are if considered as an episode in Greek moral psychology,[8] display nothing of the rigorous reasoning of a proper counterpart to harmonics. Little argument is offered to support the proposed analyses and correspondences; and one cannot help feeling that Ptolemy, in his role as a scientist, is only half-heartedly engaged in the project. Substantial parts of the section on stellar movements and configurations, by contrast, are chock-full of close mathematical detail, meticulously argued and (within limits) persuasively associated with patterns of harmonic ratios.

The only point I want to make about them, however, is a simple one. The conception of mathematical science which Ptolemy has presented is that of a capacity that does not merely analyse sets of quantitative relations, but homes in on those that are of special significance, and discovers the principles on which their significance rests. Not every configuration of the stars and planets will then be of equal interest. Now some theorists, taking their lead from Plato and the early Pythagoreans, found harmonic relations instantiated in the fundamental structure of the heavens. The distances between the spheres of the heavenly bodies, or their relative speeds of movement, or both, were ordered according to the ratios proper to a well formed musical attunement.[9] This approach, in its own way, is authentically astronomical; that is, its project is to provide an intelligible exposition of the overall ordering of the cosmos, an explanation of the reasons why it is organised in the way that it is. The major components of the universe stand in the relations they do because they jointly constitute a perfectly harmonious arrangement, a thing of intelligible beauty.

While this approach seems consistent with Ptolemy's reflections in III.3, and was certainly the kind of investigation I expected to meet in III.8–16 when I first read the *Harmonics*, it does not in fact correspond at all to the contents of those chapters. They contain nothing that explains in harmonic terms the overall structure of the heavens, and nothing that would provide a musicological interpretation for the astronomical observations and theories of the *Almagest*. It is not because they are fundamental to the beautiful ordering of the heavens that certain relations between celestial bodies are picked out as significant and deserving of analysis, but because they conform, at certain points in these bodies' travels, to systematically identifiable geometrical patterns. It is these passing – and from a cosmological perspective quite incidental– patterns that are provided

[8] For a stimulating discussion of this facet of the matter see Long (1991).
[9] See e.g. Ar. *De caelo* 290b12 ff. with *Metaph.* 985b23 ff., Plato *Timaeus* 34b-36d, Nicomachus *Ench.* ch.3.

with analyses in terms proper to harmonics, and are thus assigned special meaning and importance.

Only one diagnosis of Ptolemy's strategy fits the facts. What he means by *astronomia* in these chapters is not what we would call 'astronomy', but astrology.[10] Harmonics gives us a key to the significance, for us on earth, of the configurations adopted by the stars and planets. In Ptolemy's time there was nothing disreputable or unscientific about this pursuit, and Ptolemy was himself the author of a remarkable treatise on the subject, the *Tetrabiblos*, which defends the science's credentials with enthusiasm and ingenuity. The puzzle we are left with is not that Ptolemy, as a scientist, took astrology seriously. It is that the *Tetrabiblos* pursues its astrological agenda without the least indication that the foundations of its investigations might lie in the science of harmonics.[11] It is a pretty problem, but one that I shall leave for others to resolve.[12]

[10] For another (thoroughly idiosyncratic) treatment of the subject with unmistakably astrological overtones see Aristides Quintilianus III.21–3.

[11] Harmonic considerations are indeed invoked occasionally, but only to explain the importance of a few specific configurations (e.g. *Tetrabiblos* 1.13 on 'opposition'), not as the basis of the enterprise as a whole.

[12] On the subject in general, see Barton (1994). She alludes (p. 113) to the eclipse of musical approaches to astrology by a more Aristotelian paradigm, attributing responsibility for the change of direction primarily to the influence of Ptolemy. But it is evidently only the *Tetrabiblos* that she has in mind at this point in her discussion. She does not mention Ptolemy's apparent adoption of a 'musical' model for the science in the *Harmonics*, or raise the question why the doctrines of this treatise play so marginal a role in the *Tetrabiblos* itself.

Bibliography

ABBREVIATIONS

DK = H. Diels and W. Kranz, *Die Fragmente der Vorsokratiker*, 8th edition (Berlin 1956–9).
GMW1, *GMW2* = A. Barker, *Greek Musical Writings* vols. 1–2 (Cambridge 1984–9).
MMG = L. Zanoncelli, *La manualistica musicale greca* (Milan 1990).
MSG = C. von Jan, *Musici scriptores graeci* (Leipzig 1895).

TEXTS AND TRANSLATIONS

Alypius, *Eisagoge mousike*, in *MSG* and *MMG*.
Anonymus Bellermanni, *De musica* ed. D. Najock (Leipzig 1975).
Aristides Quintilianus, *De musica* ed. R. P. Winnington-Ingram (Leipzig 1963); translation in *GMW2*.
Aristotle, *De generatione animalium, De anima, De caelo, Metaphysics, Physics,* in the Oxford Classical Texts; translations of relevant passages in *GMW2*.
[Aristotle] *De audibilibus* = Porphyry, *Commentary* 67.24–77.18; translation in *GMW2*.
[Aristotle] *Problems* ed. W. S. Hett (London 1970, Aristotle vol. 15 in the Loeb Classical Library), with translation; see also F. Gevaert and J. C. Vollgraff, *Problèmes musicaux d'Aristote* (Ghent 1903); texts also in *MSG*; translations in *GMW1* and *GMW2*.
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